Summer 8-2021

Essays on Pro-Poor Education Policies and Tax Compliance

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ABSTRACT

ESSAYS ON PRO-POOR EDUCATION POLICIES AND TAX COMPLIANCE

BY

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AUGUST 2021

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Major Department: Economics

This dissertation consists of three essays and fits in the broader literature of how to improve public finances in a context of increasing use of electronic means of payments and how to efficiently allocate scarce public resources to development programs. Analyzing the latter, the first essay evaluates the impact of the 2008-2009 universal primary education policy in Togo on school outcomes while the second essay investigates the effect of the same policy on child labor. Finally, the third essay examines whether the growing reliance on electronic means of payments stimulate public revenues by assessing the impact of plastic money use on value-added tax (VAT) compliance in the European Union (EU).

Entitled “A Tale of a Comprehensive Pro-Poor Education Policy?”, the first chapter sheds light on the causal effects of free primary education policies implemented in quite a few African countries. Using administrative data on the universe of school districts in Togo from 2005-2017, I leverage differences in pre-policy enrollment rates across local areas to identify the impact of the policy. I find that the policy significantly spurred primary school completion and graduation, more so in areas with low baseline enrollment rates. The graduation rate was not affected, suggesting a

non-deterioration in education quality. A likely reason is that school openings and teacher hiring increased consistently with the surge in enrollment. Moreover, districts with a rising supply of schools and teachers recorded higher number of final graders and graduates. It also appears that the intervention has not crowded out private school outcomes.

As for the second chapter, titled "Does Free Primary Schooling Curb Child Labor? Evidence from Togo", inform the empirical literature on the causal effect of a nationwide suppression of public primary school fees on child labor using as case study the 2008-2009 universal primary education policy (UPEP) in Togo. Using data from the 2006 and 2010 waves of the Multiple Indicator Cluster Survey (MICS) in a difference-in-differences approach that compares potential beneficiaries of the UPEP to non-beneficiaries before and after the policy, I find that the policy would curb the propensity to child labor by about 4-5 percentage point. This result is mainly driven by a decline in children's engagement in non-productive activities. However, the policy has not affected children's likelihood to perform economic activities. This finding suggests that a free primary education policy is not sufficient to reduce some types of child labor even though it effectively increases school attendance.

Finally, the third chapter analyzes the impact of credit and debit card usage on consumption tax compliance using annual national level data on 26 European Union countries from 2000 to 2016. Exploiting spatiotemporal variation in plastic money use along with an instrumental variables approach, this study finds that a 1% increase in card payments reduces VAT gap by 0.51 percentage point whereas a 1% increase in cash withdrawals increase VAT gap by 0.6 percentage point. This paper's contribution lies in using more adequate measures of VAT compliance gap and in accounting for potential confounders including *ex-ante* enforcement capacity of tax administrations.

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BY

AMAKOE DELALI ALOGNON

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
Doctor of Philosophy
in the
Andrew Young School of Policy Studies
of
Georgia State University

GEORGIA STATE UNIVERSITY

2021

ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Dedication

In loving memory of my mom, *Elise*.

To *Rebecca*, *Lily Pearl*, and *Elise Rose*.

To all the men and women who believed in my potentials and graciously invested in my personal and professional development.

Acknowledgements

"*If I have seen further, it is by standing on the shoulders of Giants*." Aware of this quote of Sir Isaac Newton, I would like to express here my profound gratitude to my advisors for their support, constructive criticism, and suggestions. I would like to specially thank my principal advisor, Jorge Martinez-Vazquez, who believed in my potentials and whose great humility, knowledge, and experience have inspired me all these years. I also want to acknowledge Pierre Nguimkeu who has been like an elder brother to me and provided me with advice and insights throughout this journey. May I also be allowed to thank Alberto Chong and Charles Hankla for their invaluable comments and suggestions that contributed to raise the stance of this thesis. I am also particularly grateful to Dan Kreisman, Jim Marton, and Tom Mroz for their useful comments and help although they were not members of my dissertation committee.

Finally, I would like to express my gratitude to Georgia State University and the Andrew Young School for the financial support of this dissertation as well as other scholarships. My research and studies would not have been smooth without the help and prompt responsiveness of the administrative staff, especially Bess Byler and Kristy Hill. Thank you!

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Chapter 1. A Tale of a Comprehensive Pro-Poor Education Policy?

1.1. Introduction

Education through human capital building has widely been viewed as a key driver to poverty alleviation and economic development. However, access to education is limited in Sub-Saharan Africa even at such a basic level as primary education. Several studies have attributed this shortcoming to financial constraints (Petrosino et al., 2012; Krishnaratne et al., 2013; Murnane & Ganimian, 2014). In this regard, and in order to achieve universal primary education, quite a few developing countries have recently reduced or eliminated school fees as those fees have been found to be a major deterrent to educational access (Holla and Kremer, 2008). In this context, the Togolese government has also suppressed public primary school fees from the academic year 2008-2009 onward. What is the impact of this policy on demand- and supply-related school outcomes?

Put in the socioeconomic context, this is a major pro-poor educational reform and, as such, it can be saluted if one recalls that 61.7% of Togolese were living with less than a dollar per day in 2006 according to the National Institute of Statistics (INSEED). However, the widespread extension of education to all social classes can be detrimental to education quality by adversely affecting educational outcomes if classrooms construction, teachers hiring, and other accompanying measures do not follow the surge in enrollment. In fact, overcrowded classrooms would render teaching more challenging since it would be quite difficult for teachers to monitor each student's progress. This, combined with the negative peer effect due to classrooms' overpopulation, would undermine student's performances. Therefore, the intervention could spur the movement of children from less poor families into private schools (Hsieh and Urquiola, 2006)

in anticipation of a depreciation in education quality due to the impending risk of public school classrooms being overcrowded. Overall, the combination of these effects would be reflected in the number of students in public and private schools and in their performance on the primary school exit exam. The effects on private school supply may also be ambiguous. In fact, while the intervention may spur the movement of students from less disadvantaged backgrounds into private schools, which may increase the private supply of schools and teachers, it may also introduce some distortions in the school supply market by causing some private schools, probably the least expensive ones, to close and teachers to be dismissed.

On the other hand, in a context of limited public resources, governments and taxpayers need to know whether these policies are effective or if they should be forgone in profit of other relevant policy alternatives which may be more cost-effective. Several development projects carried out in developing countries have been financed through increasing and relentless public indebtedness. The fact that some of these programs fail or have adverse effects relatively to the intended ones constitutes a threat to development insofar as the invested funds must be reimbursed or could have been allocated to other more promising projects.

In consequence, this study has an important implication for the broader literature on public finance and development. In this respect, there has been question as to the efficacy of the universal primary education policy (UPEP) initiated by the Togolese government in the school year 2008-2009. However, to the best of my knowledge, no impact evaluation study has been conducted so far on this significant pro-poor policy both in the specific context of Togo as well as in the general context of West and Francophone Africa. The investigation is all the more important and topical and deserves attention if one recalls that just last year, the Democratic Republic of Congo also implemented the UPEP. This study purports to fill that void by approaching the problem from an

econometric perspective, thereby contributing qualitatively and quantitatively to the literature on the effect of such reforms on school outcomes in West and Francophone Africa.

In fact, the existing literature on Eastern African countries (Lucas and Mbiti, 2012; Grogan, 2009; Nishimura et al, 2008; Al-Samarrai and Zaman. 2007; Deininger, 2003) is mainly correlational. In a nutshell, those studies find that free primary education programs have increased enrollment, particularly among students from poorer families, reduced the incidence of delayed primary school entry, but also led to a decline in the quality of education. However, as noted by Lucas and Mbiti (2012), those studies have mostly relied on cross-cohort differences in exposure. Therefore, they might have wrongly attributed unrelated changes over time to the free education program.

Moreover, by excluding private schools from their analyses, some of the existing studies abstracted from the potential sorting between public and private schools that might have occurred subsequently to such interventions. In this respect, this paper makes an important methodological contribution to the existing literature by providing causal evidence of the impacts of such policies. By using quasi-experimental methods for identification and by checking the robustness of the results to alternative specifications that properly account for the data generating process, this study provides reliable causal estimates of FPEPs. I leverage, in a difference-in-differences and triple difference settings, changes over time across public and private schools as well as spatial variations in the intensity of treatment across local areas to disentangle the effect of the intervention. Unlike previous works, this paper looks beyond enrollment outcomes and investigates ultimate demand-related outcomes such as final grade attainment and graduation as well as supply side outcomes, namely, school openings, classroom constructions and teachers' hiring.

Second, it seeks to evaluate whether there are sizable differences in the effects across local areas and gender. In fact, the aforementioned pre-existing socioeconomic conditions are exacerbated in the northern part of the country where coexist highest poverty rates, largest sizes of households, and lowest net primary enrollment rates. For example, in 2006, 86.7% of the population in the Savanes region were living with less than a dollar per day, 74.2% in the Kara region, and 74.6% in the Central region with average sizes of households reaching 6 to 7 persons in 2011 (INSEED). These localities were also the ones recording the lowest net primary enrollment ratios which are 52.6% in the Savanes, and 64.1% in the Kara compared to 80% in the Maritime region (INSEED). Given that some local areas were exhibiting relatively low rates of demand for schooling prior to the intervention, they will be likely more intensely affected by the reform. It then appears important to evaluate the differential effects of the intervention across local areas.

Moreover, some past cultural beliefs regarding girls' schooling have not completely disappeared, and a financially constrained family may have preference over educating their boys than the girls. This is plausible in rural areas. Here lies the interest of examining the effects on the demand for schooling across gender. In order to tailor comprehensive policy recommendations, this paper, unlike earlier studies which focused mainly on demand-related outcomes, further investigates the effect on the supply of schools and teachers and evaluates whether the impact of these reforms depend on variations in the supply. By ignoring supply side responses, many previous studies would have occulted the stimulative or distortive outcome of such interventions on private school supply.

To address these questions, I use administrative records of the Togolese Ministry of Primary and Secondary Educations on the universe of school districts from 2005 to 2017 as well as the 2010 population census metadata released by the National Institute of Statistics. I find that

the program has substantially enhanced access to the final grade of primary school as well as the number of graduates without affecting the graduation rate. The number of final graders and graduates have significantly increased, more so in prefectures with low baseline enrollment rates, suggesting a narrowing of the gap in primary school attainment and graduation between children from different wealth backgrounds. The effects by gender imply a waning of the gap between boys and girls. There is no significant change in graduation rates, suggesting that the intervention has not deteriorated public education quality. This result can be due to consistent improvements in school openings and teachers hiring, more so in intensely treated areas. I also find that school districts that increase their supply of schools and teachers recorded higher number of final graders and graduates with, once again, no significant impact on the graduation rate. It also appears that the intervention has not crowded out private school supply or demand outcomes.

The remainder of this paper is organized as follows. The next section presents a brief review of the literature on the effects of free education policies in Africa. The third section succinctly describes the primary education sector in Togo and the context that prevails to the implementation of the free public primary education policy. Section 4 presents the data and summarizes the key variables before and after the reform. Section 5 describes the empirical methodology. Section 6 exposes the various robustness checks conducted. Section 7 discusses the results while the last section concludes with some policy implications.

1.2. Literature Review

Few studies have investigated the effect of school fees cancellation on student performance in Sub-Saharan Africa. Malawi has been a pioneer in implementing free primary education policy in 1994. Evaluating the impact of that policy from a descriptive point of view, Al-Samarrai and

Zaman (2007) find that enrollment rates have increased dramatically over the 1990s, at both the primary and secondary levels, and that these gains have been greatest for the poor.

Uganda was also among the first Sub-Saharan Africa countries to implement a free primary schooling program in 1997. The analysis of the effects of this policy received ample attention. Deininger (2003) finds that the universal primary education program in Uganda was associated with an increase in primary school attendance; inequalities in attendance related to gender, income, and region were substantially reduced; and school fees paid by parents decreased at the primary but not at the secondary level. He also noted a general decline in the quality of education. Likewise, using data on 940 rural households, Nishimura et al. (2008) find that the Ugandan UPEP decreased delayed enrollments and spurred grade completion rates up to the fifth grade, and the effects are especially pronounced among girls in poor households. In the same vein, Grogan (2009) found that this policy conducted by Uganda has boosted by 3% the probability that a child begins school before being 9 years old.

A common shortcoming to these studies lies in the use of cross-cohort differences in exposure, which cannot provide rigorous and reliable estimates of the impact of these policies. Besides, they did not examine variations in the supply side, which can help formulate adequate policy recommendations.

In Kenya, Lucas & Mbiti (2012) find that the elimination of primary school fees increased the number of students who took primary school exit exam, spurred private school entry, and boosted access for students from disadvantaged backgrounds. They argue that the program was welfare enhancing as it promoted educational access without substantially reducing the test scores of students who would have been in school in the absence of the program. Though causal, this study has some limitations. This study can be viewed as complementary to Lucas and Mbiti' work

for the following reasons. First, since information on graduates are not available, they cannot confidently extrapolate the effect on graduation. Having more test takers as results of the policy does not necessarily imply an improvement in the number of graduates and even less in the graduation rate, which are probably the ultimate demand-related target of the policy. Second, they did not consider supply side variables such as teacher hiring and classroom construction. The present paper has the advantage of analyzing those outcomes. Third, they did not investigate how the demand for schooling reacts to policy-induced changes in supply side variables. This paper fills that void by evaluating how growth in school openings and teachers hiring affects student outcomes.

To sum up, these studies find that free primary education programs increased enrollment, particularly among students from poorer families, reduced the incidence of delayed primary school entry and led to a slight decline in education quality. Fewer policies and studies have focused on secondary schools.

Gajigo (2012) and Blimpo et al. (2015) estimate the impact of girls' scholarship program in the Gambia. The program suppressed school fees for female students attending public secondary schools in the implementation regions. Exploiting the gradual geographic rollout of the program across regions, they find that the policy has boosted enrollment rates without harming learning outcomes. They report a drastic increase of about 50% in the number of girls and boys taking the high school exit exam as well as a 0.1 standard deviation gain in test scores as result of the intervention. This paper shares the same limitations mentioned earlier for Lucas and Mbiti (2012).

This study adds to the literature by providing one of the first causal evidence of a large scale free primary education policy on ultimate student outcomes such as primary completion and graduation. It also goes beyond demand side responses and investigates the effect on school and

teacher supply as well as the effect of supply variations on student outcomes in order to provide a more complete view of the impacts of the reform.

1.3. Overview of the Primary Education Sector in Togo and Policy Context

Togo's education system is divided into four levels: a three-year pre-school cycle designed for 3-5 years old, a six-year primary cycle designed for 6-11 years old, a seven-year secondary education cycle designed for 12-18 years old, consisting of a four-year junior level and a three-year senior level and a higher education system (two public universities and private institutions). There are also technical and vocational education at the junior and senior secondary levels and literacy programs. The primary cycle education is provided by public and private schools and recently also by community schools (EDIL[1]) with teachers paid by parents. To graduate from primary school and earn a primary school diploma, students must take and pass the national primary school exit exam at the end of grade 6.

Over the last decade, Togo has experienced high levels of enrollment compared to the observed average in sub-Saharan Africa. However, the sector is confronted to some issues. First, the primary completion rate (PCR) was the same in 2009 as it was in 1999 (World Bank, 2010). Second, the education system is also experiencing a high level of dropouts that negatively affect the completion rate. Only 61% of new entrants complete the primary cycle compared to 71% on average in Sub-Saharan Africa (World Bank, 2010). There is a noticeable discrepancy in primary completion rates between boys and girls as well as between rural and urban areas. In fact, in 2006,

[1] French acronym for Ecole d'Initiative Locale.

the PCR for girls was only 61% versus 76% for boys. Disparities between rural and urban areas and between levels of family income were even larger. PCR in rural areas was only 60% compared to 84% in urban areas. Strong inequalities in completion rates around the national average also prevail across the administrative regions. Indeed, the PCR in 2007 ranged from 48% in the Savanes region to 95% in the Lomé-Golfe region (Déclaration de Politique Sectorielle, 2009). Lack of classrooms and the cost of schooling for households are considered to be the forces driving pupils to drop-out.

In order to tackle these problems, the Government adopted a sector development policy, a 2010-2020 Education Sector Plan (ESP) and a 2010-2012 Sector Medium Term Expenditure Framework (MTEF) for the three first years of ESP implementation. The ESP was officially endorsed by all the technical and financial partners and allowed Togo to be admitted to the Education for All-Fast Track Initiative Catalytic Fund (EFA-FTI). Togo received 45 million US dollars to support the implementation of the first phase (2010-2012) of the Plan. The ESP's priority objective is to achieve universal primary education by 2020. Aware of the issues faced by the primary education sector and in order to make a substantial progress in the Millennium Development Goals by achieving the universal primary schooling by 2015, one year before the enactment of the ESP, the Togolese government abolished school fees for public schools at pre-primary and primary levels. A public financial compensation is provided each year to counterbalance the loss of resources for schools. Since about 60% of the population was living below the poverty line, the policy would have certainly helped reduce school fee constraints for households and affected access to school for children from poor family backgrounds.

1.4. Data and Descriptive Statistics

To assess the impact of the free primary education on student outcomes and social inequality in Togo, I use records of the Togolese Ministry of Primary and Secondary Educations on the universe of school districts of the country. The district is the finest level of disaggregation that is used for administrative and planning purposes by the Ministry of Primary Education. These administrative records are school type-district level data that cover the period 2005-2017 and provide information on the type of school (private, public, or EDIL), students' enrollment at each of the six grades of elementary education, the number of primary school exit exam test takers, the count of those who pass the exam, the count of private/public schools per district, the number of classrooms per type of school, the count of teachers per type of school as well as their qualifications, etc. Due to the peculiar features of the EDIL and given that they are relatively negligible with respect to public and private schools and because they are absent in most of the districts, they are abstracted from in this study. From 44 school districts in the academic year 2004-2005, the number of districts rose to 61 in 2012 mainly as result of splitting or extending or even reducing the jurisdiction of existing districts. Therefore, the overseeing district changes several times for some schools. Thus, for consistency, and in order to calculate the treatment intensity variables, the observations are collapsed for some districts. In all, there are roughly 1100 school type-district-year observations nested in 41 districts. To compute gross enrollment rates defined as the number of enrollees divided by the primary school-aged population, the study uses data from the 4^{th} Population Census conducted in 2010 by the National Institute of Statistics and Economic and Demographic Studies (INSEED). Theoretically, the population aged 6-11 years is considered as the primary school-aged population. However, the data show that about 98% of primary school pupils belong to the age group 5-14 years. Therefore, I consider rather the latter

age group to compute the gross enrollment rates. The results are however the same if one uses instead the 6-11 years old population. The administrative subdivision of the country in the census is the prefecture which encompasses potentially one to three school districts. Hence, population data are available at the prefecture level. As of 2017, the 61 school districts were nested in 36 prefectures. Two localities, namely Anie and Mo, which were respectively part of the prefectures of Ogou and Sotouboua during the pre-intervention period have been erected in prefecture in 2012. The post-reform observations on these newly created prefectures are collapsed with those of the prefectures they formerly belonged to. The sample size shrinks to 880 school-type-prefecture-year observations when using prefecture-level data. Table 1 presents summary statistics for public and private schools before and after the policy.

Table 1.1. Summary Statistics for Key Variables Before and After the Reform

Variable	**Before (2005-2008)**	**After (2009-2017)**	**Percentage change (%)**
Number of enrollees			
Public schools	13,491	21,053	56.1
Private schools	7,252	8,467	16.8
Number of 6^{th} graders			
Public schools	1,645	2,319	41.0
Private schools	965	1,096	13.6
Number of test takers			
Public schools	1,608.07	2,090.33	30.0
Private schools	951.21	1,014.40	6.6
Number of graduates			
Public schools	1,151	1,658	44.0
Private schools	745	877	17.7
Number of schools			
Public schools	61	93	52.5
Private schools	44	43	-2.3
Number of schools per student			
Public schools	0.005	0.005	0.0

Private schools	0.007	0.006	-14.3
Number of classrooms			
Public schools	328	484	47.6
Private schools	227	245	7.9
Number of classrooms per student			
Public schools	0.025	0.024	-4.0
Private schools	0.031	0.03	-3.2
Number of teachers			
Public schools	312	467	49.7
Private schools	225	250	11.1
Number of teachers per student			
Public schools	0.024	0.023	-4.2
Private schools	0.031	0.031	0.0
Sample size	351	817	
Public schools	176	413	
Private schools	175	404	
Non-enrollment rates			
Mean	37.62	16.74	-55.5
Median	34.06	16.02	-53.0
Sample size	272	612	

Note: Counts are calculated as annual averages at district level over the relevant period.

While there are noticeable differences in outcomes for public schools before and after the intervention, changes for private schools are not substantial. It can be noted an increase in demand side variables in public schools after the implementation of the policy. In fact, on average, from 2009 to 2017, 56% more pupils enrolled in public school compared to the period 2005-2008, 41% more attained the final (6^{th}) grade of elementary school relatively to the pre-intervention period, 30% more took the primary school exit exam, and 44% more graduated from primary school. Potential supply responses to the policy are the construction of new schools and teacher hiring. Table 1 also shows noticeable increases in the average number of public schools and public school teachers after the enactment of the program. In fact, the count of public schools increased by 53% while the number of teachers rose by almost 50% relatively to the baseline level. Overall, the total

number of enrollees, 6th graders, test takers, graduates, schools, and teachers increased for public schools after the free primary education policy was introduced in the school year 2008-2009. It is worth noting that the count of schools per enrollee and the number of teachers per enrollee did not change noticeably, which is a potential safeguard against a deterioration in public school quality. Besides, the count of graduates in public schools increased as well, suggesting that the rise in enrollment does not necessarily lead to a depreciated public school quality. Figures 1.1 to 1.4 below show the evolution of school demand outcomes from 2005 to 2017.

Figure 1.1. Evolution of the Average Number of Enrollees by School Type

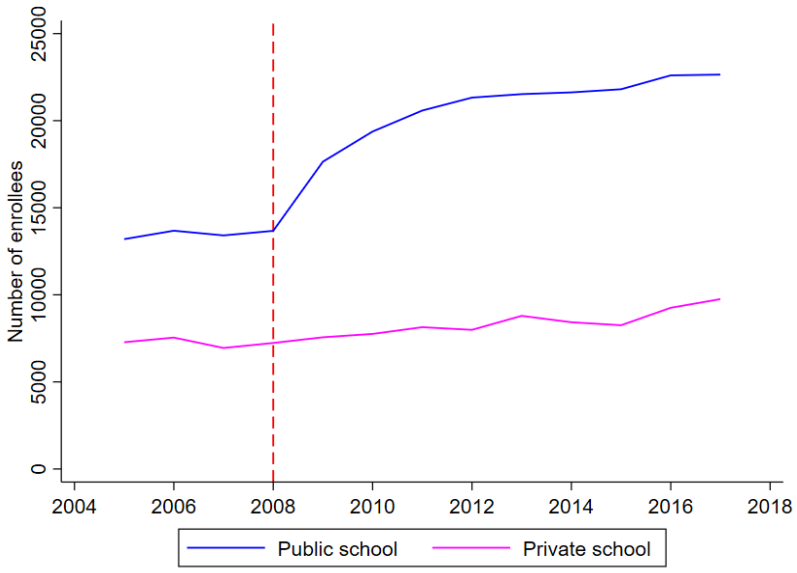


Figure 1.2. Evolution of the Average Number of 6th Graders by School Type

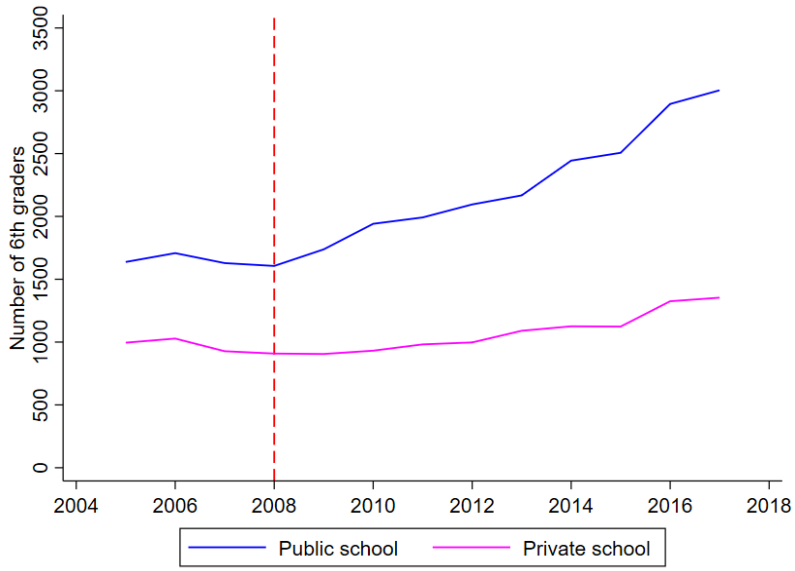


Figure 1.3. Evolution of the Average Number of Graduates by School Type

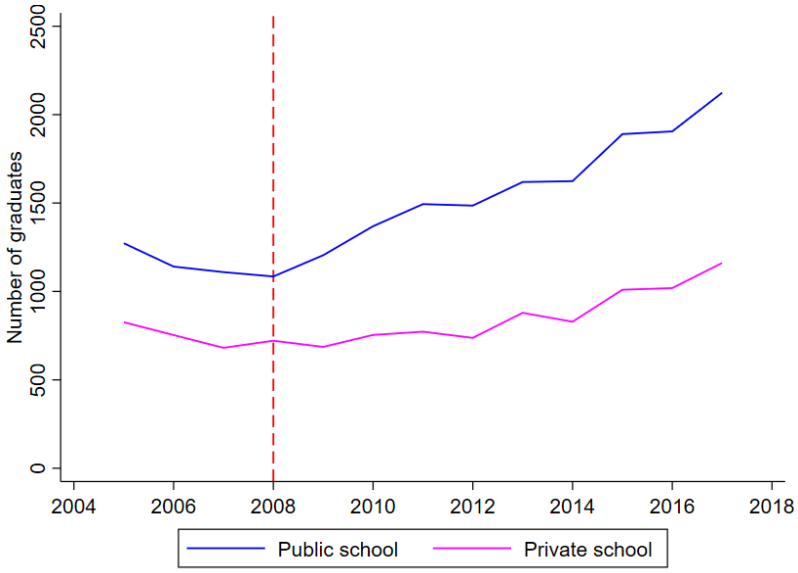
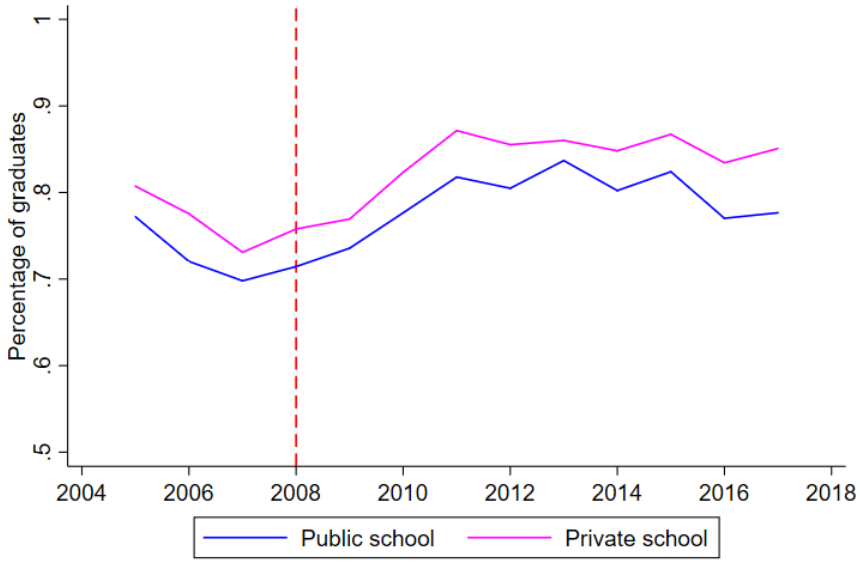
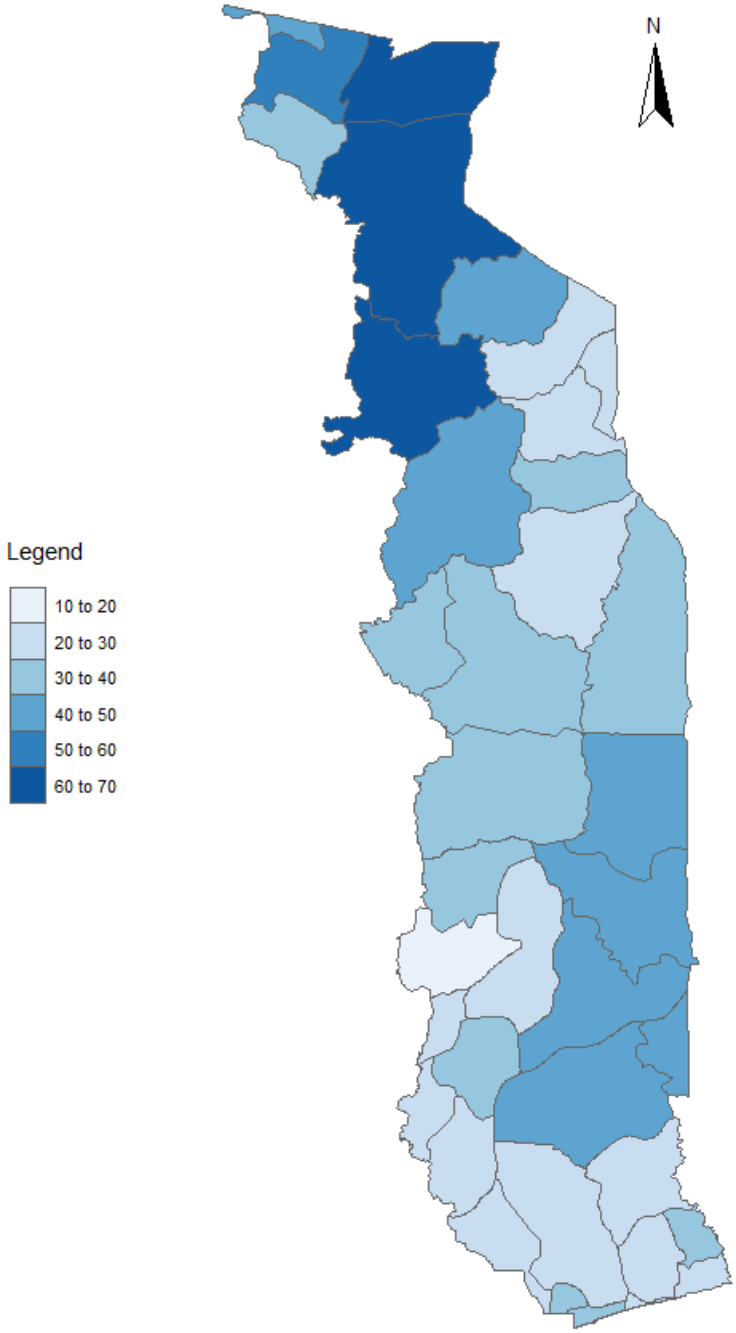


Figure 1.4. Evolution of the Graduation Rate by School Type



There have been also remarkable variations in the non-enrollment rates. The average non-enrollment rate was about 38% before the policy and declined to nearly 17% afterward, corresponding to a 55.5% drop. The median non-enrollment rate shrinks also consistently by 53%. Figure 1.5 below illustrates the variations in the pre-policy non-enrollment rates across prefectures. Darker colors depict higher pre-policy non-enrollment areas, which are likely more intensely treated.

Figure 1.5. Non-Enrollment Rate Across Prefectures in 2008



1.5. Empirical Strategy

In order to capture the effect of the free public primary education policy on school outcomes (6^{th} grade attainment, graduation, graduation rate, etc.), this paper adopts two distinct strategies. First, it abstracts from the possible substitution between public and private school as result of the intervention. In fact, some households at the margin of the income distribution may remove their children from public schools in anticipation of a depreciation of the learning environment due to the impending risk of overpopulation of classrooms. Likewise, children attending the least costly private schools may be redirected by their parents to the henceforth free public schools. However, since the dataset is not longitudinal at the student level (but rather at the school type-district level), this partial compliance issue cannot be properly addressed. Besides, as shown by figures 1.1 to 1.4 above, private school outcomes have been fairly stable across time, a suggestive evidence of a moderate sorting across public and private schools. A necessary assumption for the difference-in-differences (DD) model to recover causal estimates is that public schools and their private counterparts would follow the same trend in the post-intervention period, had the public schools not been made free of charge. However, this assumption is untestable since the researcher does not observe the counterfactual. The empirical econometric literature instead provides evidence on this assumption by evaluating whether pre-reform trends are parallel. Figures 1.1 to 1.4 provide visual supports of common pre-policy trends in unconditional mean outcomes. To statistically test for parallel trends, I compare trends in outcomes across school types over the pre-reform period (2005-2008). A non-statistically significant interaction term between the treatment indicator and the linear (respectively non-linear) time trends in equation (1.1) would suggest common pre-reform trends.

$$y_{sdt} = \beta_0 + \beta_1 Public_s + \beta_2 Public_s * trend_t + X_{sdt}\alpha + \delta_d + \tau_t + \epsilon_{sdt} \quad (1.1)$$

y_{sdt} is the outcome (number of 6th graders, number of graduates, and graduation rate) for school type s (either public or private) in district d in year t. $Public_s$ is the treatment indicator equal to 1 if school type s is public. $trend_t$ is either a linear time trend or the set of pre-reform year dummies. X_{sdt} represents a set of supply-side variables that may change across school, district, and time such as the number of school per student, the count of classroom per pupil, the number of teacher per classroom, etc. δ_d represents district-specific time-invariant effects on school outcomes. τ_t captures year-fixed effects such as nationwide social, political, and economic conditions that affect indifferently schools and susceptible to blur the effect of the free primary education policy. ϵ_{sdt} is the error term assumed to have the usual statistical properties.

I also explore another way to test for similar pre-policy trends between public and private schools. Following Slusky (2016), I conduct a placebo test over the pre-intervention period and define the placebo treatment to occur in 2006-2007. The idea is to rule out spurious effects that would be mistakenly imputed to the policy. If the common pre-reform trend assumption is validated, the difference-in-differences model to be estimated can be specified as follows:

$$y_{sdt} = \beta_0 + \beta_1 Public_s + \beta_2 Public_s * Post_t + X_{sdt}\alpha + \delta_d + \tau_t + \epsilon_{sdt} \quad (1.2)$$

where all the variables are defined as previously. $Post_t$ is an indicator variable equal to 1 for every post-intervention year and 0 otherwise. Note that $Post_t$ is not included in the model except in the interaction term since it is perfectly subsumed in the year fixed effects. Standard errors are clustered by district to account for correlation in the district-level errors over time. In this setting, β_2 represents the DD estimate of the impact of the program on public schools relatively to their private counterparts.

Given potential concerns about the identifying assumptions in the previous setting, in this approach, I leverage the additional layer of geographic variations in the intensity of treatment arising from differential pre-policy enrollment rates across prefectures to estimate a difference-in-differences (DD) and a triple difference (DDD) models. Figure 1.5 provides a visual support of these variations. The underlying idea is that even though the policy was conducted nationwide, its intensity in terms of enrollment would be stronger in prefectures which had low baseline enrollment rates. Areas with relatively high pre-reform enrollment rates would potentially experience a smaller rise in enrollment than others with lower pre-intervention enrollment rates. However, the ultimate effect on 6th grade attainment and graduation is undetermined. In fact, if school openings, teachers hiring, and other accompanying measures do not follow the surge in enrollment in low pre-treatment enrollment areas, the policy may end up subverting students' learning and the subsequent outcomes. This approach compares pupils from different pre-intervention enrollment intensity areas before and after the intervention. Finkelstein (2007) uses a similar approach to identify the impact of Medicare on health care spending. Miller (2012) also adopts this strategy to estimate the effect of the 2006 Massachusetts health reform on emergency room utilization without control units. Finally, Courtemanche et al. (2017) uses the same trick to analyze the early impacts of the Affordable Care Act (ACA) on health insurance coverage in Medicaid expansion and non-expansion states. The model can be specified as follows:

$$y_{spt} = \beta_0 + \beta_1 Z_p * Post_t * Public_s + \beta_2 Z_p * Post_t + \beta_3 Public_s * Post_t + \beta_4 Z_p * Public_s + X_{spt}\alpha + Public_s + \delta_p + \tau_t + \epsilon_{spt} \quad (1.3)$$

where y_{spt} denotes the outcome for school type s (either public or private) in prefecture p in year t; Z_p is the pre-program prefectural nonenrolment rate; X_{spt} represents a set of control variables;

captures school type specific effects; δ_p denotes time-invariant local area unobserved heterogeneities; τ_t captures year-fixed effects; ϵ_{spt} represents the idiosyncratic error term, and all the remaining variables are defined as before. Note that $Post_t$ and Z_p are not included separately in the model except in the interaction terms since they are perfectly collinear to the year-fixed effects and prefecture-fixed effects, respectively. In this specification, the impact of the policy is assumed to vary linearly with the baseline non-enrollment rate. Standard errors are clustered by prefecture to account for plausible correlation in the prefecture-level errors over time.

The identifying assumption in this DDD model is that, in the absence of the free primary education policy, any changes that would have occurred in public and private school outcomes would not have varied differentially by prefectural non-enrollment rates, which is a weaker assumption than that of the first approach. In fact, this approach does not need to assume that public and private schools share common counterfactual trends. It only assumes that, to whatever extent differential trends may exist, the difference is not related to pre-reform non-enrollment rates. To provide suggestive evidence to the validity of this assumption, I evaluate pre-reform trends in outcomes across school types and areas with different baseline enrollment rates. I check once again the sensitivity of these results by conducting a falsification test that creates a fictitious policy intervention in 2006-2007.

To evaluate the differential effect of the reform across gender, equations (1.1) to (1.3) are separately estimated for boys and girls.

I also exploit district level variations in the supply side to check whether the abolition of fees has differential effects on school outcomes depending on the intensity of school openings and teachers hiring. Before the intervention, the intensity of supply is zero in all districts as no school

construction or teacher hiring occurred as result of the free primary education policy. After the policy, the supply intensity within a district is the percentage deviation of the supply from its pre-reform level. To assess how this policy-induced supply growth affects school outcomes, I estimate the following model:

$$y_{sdt} = \beta_0 + \beta_1 Intensity_{dt} + \delta_d + \tau_t + \epsilon_{sdt} \quad (1.4)$$

where all the variables are defined as previously, $Intensity_{dt}$ denotes the supply intensity variable (either variations in school openings or teachers hiring) in district d in year t.

1.6. Robustness Checks

The first check performed consists in adding control variables to the different models that evaluate the impact on demand for schooling. These controls are mainly supply variables, such as the number of schools per enrollee, the count of classroom per student, and the count of classroom per teacher, are likely to be affected by the policy. Not controlling for these variables can either understate or exaggerate the effect of the intervention. I check the sensitivity of the results to the inclusion of these variables. The results are consistent to this manipulation. Second, I control for district linear trends as well as school type-district fixed effects. The former accounts for potentially confounding existing trends across school districts while the latter controls for school type-by-district unobserved heterogeneities such as time-invariant unobserved conditions of each district's public schools. Since all the outcomes except the graduation rate are over-dispersed count data, the third specification check repeats all the analyses conducted in section 7.1 by running robust Poisson and negative binomial regressions rather than log-linear models. The Poisson specification assumes that the data is equidispersed, i.e., its mean and variance are equal. However, because these outcomes are overdispersed, the Poisson regressions may perform poorly. One way

to deal with this feature is to estimate a negative binomial model which generalizes the Poisson model to handle overdispersion (Cameron & Trivedi, 2005). For concision, and because the Poisson model is a specific case of the negative binomial one, only the results of the latter are reported. The results of the former are available upon request. Tables A.3 and A.4 in Appendix A present the estimates of the effects of the program using private schools as control. The estimates are remarkably close if not equal to those obtained from the log specifications shown in Tables 1.2 and 1.3.

I also examine the sensitivity of the prefecture level analyses by adding as previously control variables to all the specifications. Second, I estimate as earlier robust Poisson and negative binomial models. Tables A.7 and A.8 in Appendix A show that the results are similar to those reported in Tables 1.4 and 1.5. Third, I consider a first different way of constructing the pre-treatment intensity variable by using average prefectural pre-reform non-enrollment rates rather than the 2008 prefectural non-enrollment rates. Tables A.9 and A.10 in Appendix A show that the estimates are quite close to those obtained earlier implying that the results are robust to this manipulation. Finally, I consider a second different way of constructing the local area pre-policy non-enrollment rates by using as denominator of the fraction the theoretical 6-11 years old population rather than the 5-14 years age group. The results are consistent with those found in subsection 1.7.2 and are available upon request.

1.7. Results

This section discusses the results of the free primary education policy on school demand and supply outcomes. I start with the results derived from the first identification strategy and end with those obtained using the second methodological approach.

1.7.1. Effect Using Private Schools as Control Units for Public Schools

Before presenting the results on the impact of the policy in the first setting, I provide suggestive visual and statistical evidence of the common pre-reform trends assumption. Figures 1.1 to 1.4 above suggest that public and private schools exhibit the same trends in unconditional mean outcomes on the period 2005-2008 preceding the enactment of the policy.

Table A.1 in Appendix A provides formal statistical tests of the parallel trend assumption. The results are robust to linear and non-linear trends as well as to controlling for various confounding factors. In all the cases, the interaction term between the treatment variable (public school) and either the time trend or the year dummies are not statistically significant. Thus, the null hypothesis that public and private schools followed similar trends in outcomes in the pre-intervention period cannot be rejected. This suggests that the DD estimates obtained from the regressions below can be interpreted causally.

To evaluate the robustness of these results, I perform a falsification test by creating a fictitious policy intervention in 2006-2007. Table A.2 in Appendix A suggests that "placebo" interventions during the pre-treatment period are not associated with significant changes in outcomes. In fact, the results reported in Table A.2 show that the coefficients associated with the interaction between the public school dummy and the linear time trend and the year dummy respectively are not statistically significant.

Turning to the effects of the program on school outcomes, the first three columns of Table 1.2 show that the policy spurred access to the final grade in public schools compared to their private counterparts. In all the panels, the coefficient of the interaction between the public school indicator and the post-policy dummy is statistically significant at the 1% critical level. Columns (1) to (3) of Panel A indicate that approximately 24 to 26% of pupils per district attain the final year of primary school as result of the intervention. The first three columns of Panel B and C report pretty similar effects in the subpopulations of boys and girls. It is however worth noting that a linear model of 6^{th} grade enrollment suggests a 4-percentage point larger effect in the female population.

Table 1.2. Effect on Demand Using Private Schools as Control

	Log (6th Graders)			Log (Graduates)			Graduation Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: All									
Public * Post	0.241***	0.242***	0.257***	0.139**	0.140**	0.146**	-0.007	-0.007	-0.010
	(0.054)	(0.057)	(0.056)	(0.065)	(0.067)	(0.066)	(0.010)	(0.011)	(0.010)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
District trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School type-district FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	1061	1061	1061	1061	1061	1061	1061	1061	1061
R-squared	0.800	0.973	0.974	0.762	0.931	0.931	0.435	0.538	0.540
Panel B: Boys									
Public * Post	0.244***	0.245***	0.262***	0.144**	0.145**	0.157**	-0.004	-0.004	-0.006
	(0.055)	(0.057)	(0.054)	(0.063)	(0.065)	(0.063)	(0.010)	(0.011)	(0.010)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
District trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School type-district FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	1061	1061	1061	1061	1061	1061	1061	1061	1061
R-squared	0.787	0.972	0.972	0.752	0.931	0.932	0.418	0.518	0.521
Panel C: Girls									
Public * Post	0.247***	0.248***	0.259***	0.147*	0.148*	0.143*	-0.008	-0.007	-0.013
	(0.058)	(0.060)	(0.062)	(0.073)	(0.076)	(0.077)	(0.012)	(0.013)	(0.012)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
District trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School type-district FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	1061	1061	1061	1061	1061	1061	1061	1061	1061
R-squared	0.809	0.970	0.970	0.767	0.922	0.922	0.442	0.543	0.546

Notes: Standard errors, heteroscedasticity-robust and clustered at the district level, are reported in parentheses. All regressions include public school dummy, district, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

The policy has also stimulated primary school completion. In fact, columns (4) to (6) suggest roughly 14 to 15% increase in the number of graduates relatively to the pre-policy level. Panels B and C report nearly the same effects by gender while a linear model suggest roughly a 4-percentage point higher impact for girls compared to boys. The universal primary education policy has then led to slightly narrow the gap between boys and girls in terms of final grade attainment and graduation. Due to some persistent cultural beliefs, some financially constrained households in Sub-Saharan Africa have a revealed preference in schooling boys over girls. Since primary schooling is now free of charge, it is more likely to trigger girls' enrollment and graduation, which is corroborated by these results.

However, in order to evaluate whether the policy has a deteriorating effect on education quality, I examine changes in graduation rates as well as variations in the supply of schools, classrooms, and teachers. The fact that more students are graduating compared to the pre-policy level may be misleading if the number of graduates is not reported to the size of the test taking cohort. In fact, it may be the case that more students are graduating because more are enrolling. As shown by the last three columns of Table 1.2 and suggested by Figure 1.4, the graduation rate has not changed significantly in public schools relatively to private schools after the intervention. This provides suggestive evidence that the policy has not caused public education quality to deteriorate, using the graduation rate as an indicator of education quality. Plausible explanations of this result can be found in the school market supply response to the reform.

First, the supply side can respond to the policy through increasing school infrastructures such as school and classroom construction. It is worth noting that schools opened at a higher pace than the surge in overall enrollment. In fact, the first two columns of Panel A of Table 1.3 show that 46% more public schools per district have then been constructed to meet the rising demand

for schooling. Even the number of schools per student increased significantly by 5 schools per 10,000 pupils (Columns (1) and (2) of Panel B). More classrooms have also been supplied. The two middle columns of Panel A of Table 1.3 report that classrooms in public schools increase by 32% relatively to the baseline district level as result of the FPEP. Besides, the corresponding columns of Panel B indicate that, conditional on the number of enrollees, the number of classrooms built does not change substantially after the reform. The school market supply would also logically respond to the intervention by hiring more teachers since additional schools and classrooms have been opened. In fact, the last two columns of Panel A report the recruitment of 36% additional teachers per district subsequent to the policy. Interestingly, as for the count classroom per pupil, the student-to-teacher ratio has not significantly been affected (Columns (5) and (6) of Panel B).

Table 1.3. Effect on Supply Using Private Schools as Control

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	**Log (Schools)**		**Log (Classrooms)**		**Log (Teachers)**	
Public school * Post	0.4571***	0.4591***	0.3184***	0.3204***	0.3615***	0.3637***
	(0.057)	(0.059)	(0.056)	(0.058)	(0.049)	(0.051)
District trends	No	Yes	No	Yes	No	Yes
School type-district FE	No	Yes	No	Yes	No	Yes
Observations	1061	1061	1061	1061	1061	1061
R-squared	0.748	0.970	0.760	0.963	0.755	0.962
Panel B	**School/student**		**Classroom/student**		**Teacher/student**	
Public school * Post	0.0005**	0.0005**	-0.0012	-0.0012	-0.0013	-0.0013
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
District trends	No	Yes	No	Yes	No	Yes
School type-district FE	No	Yes	No	Yes	No	Yes
Observations	1061	1061	1061	1061	1061	1061
R-squared	0.659	0.872	0.518	0.673	0.225	0.300
Pre-reform mean	0.006		0.028		0.027	

Notes: Standard errors, heteroscedasticity-robust and clustered by district, are reported in parentheses. All regressions include public school dummy, district, and year fixed effects. Significance levels: *** = 1%; ** = 5%; * = 10%.

I also examine if districts with growing supply of schools and teachers experienced higher improvements in school demand outcomes. To evaluate how variations in the supply affect school outcomes, I estimate equation (4) above. The results are reported in Table 1.4 below.

Table 1.4. Effect of Supply Variations on School Outcomes

	Log (6th Graders)		**Log (Graduates)**		**Graduation Rate**	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A						
School growth	0.0049***	0.0050***	0.0042***	0.0043***	-0.0000	-0.0000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
District trends	No	Yes	No	Yes	No	Yes
School type-district FE	No	Yes	No	Yes	No	Yes
Observations	1060	1060	1060	1060	1060	1060
R-squared	0.434	0.966	0.438	0.920	0.403	0.469
Panel B						
Teacher growth	0.0030***	0.0031***	0.0028***	0.0029***	-0.0000	-0.0000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
District trends	No	Yes	No	Yes	No	Yes
School type-district FE	No	Yes	No	Yes	No	Yes
Observations	1060	1060	1060	1060	1060	1060
R-squared	0.429	0.960	0.435	0.917	0.403	0.469

Notes: Standard errors, heteroscedasticity-robust and clustered by district, are reported in parentheses. All regressions include public school dummy, district, and year fixed effects. Significance levels: *** = 1%; ** = 5%; * = 10%.

It appears that post-policy variations in the supply of schools and teachers are associated with significant positive effects on final grade attainment and the number of graduates. This means that districts which experienced a rise in school openings and teachers hiring subsequent to the UPEP also recorded higher increases in the number of 6th graders and graduates. This confirms the intuition on the necessity to couple free primary education policies with school construction and

teacher recruitment. There is however no significant change in the percentage of graduates, which implies that the number of graduates and the test taking cohort size increase nearly by the same proportion.

1.7.2. Effect Using Differences in Pre-Policy Enrollment Rates Across Prefectures

This approach exploits, in a triple difference framework, the additional layer of variations (spatial variations) arising from differences in the intensity of treatment across local areas to tease out the effect of the intervention. The identifying assumption is that, in the absence of the free primary education policy, changes in outcomes, if any, across school types would not have varied differentially by prefectural enrollment rates. However, as aforementioned, this assumption cannot be tested since one does not observe the counterfactual. To provide suggestive evidence to its validity, I evaluate pre-reform trends in outcomes by school type across areas with different baseline enrollment rates. Table A.3 in Appendix A shows the results of the test of parallel pre-policy trends in outcomes across school types and prefectures with different pre-intervention enrollment rates. The coefficients on the interaction term of the pre-policy prefectural non-enrollment rates (*Intensity*) and the school type (private or public) are not statistically significant over the pre-policy years, suggesting that changes in outcomes across public and private schools would not have varied by prefectural non-enrollment rates if the intervention had not occurred. To check the robustness of these results, I conduct as previously a falsification test by creating a "placebo" policy intervention in 2006-2007. The estimates reported in Table A.4 in Appendix A confirm the results obtained in Table A.3. This implies that the subsequent estimates can be interpreted as the causal effects of the universal primary education policy. The following table shows the effects of the policy using the intensity of treatment across prefectures.

Table 1.5. Effect on Demand Across Prefectures and School Type

	Log (6th Graders)		Log (Graduates)		Graduation Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Intensity*Post*Private	0.0145***	0.0085	0.0102	0.0041	-0.0008	-0.0006
	(0.004)	(0.006)	(0.006)	(0.008)	(0.001)	(0.001)
Intensity*Post*Public	0.0195***	0.0180***	0.0144***	0.0131***	-0.0014	-0.0012
	(0.003)	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	880	880	880	880	880	880
R-squared	0.854	0.862	0.813	0.822	0.481	0.486
Panel B: Boys						
Intensity*Post*Private	0.0149***	0.0082	0.0110	0.0040	-0.0010	-0.0008
	(0.005)	(0.006)	(0.007)	(0.008)	(0.001)	(0.001)
Intensity*Post*Public	0.0183***	0.0165***	0.0141***	0.0126***	-0.0011	-0.0009
	(0.003)	(0.004)	(0.003)	(0.004)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	880	880	880	880	880	880
R-squared	0.843	0.853	0.804	0.816	0.467	0.471
Panel C: Girls						
Intensity*Post*Private	0.0153***	0.0101	0.0106	0.0054	-0.0006	-0.0002
	(0.004)	(0.006)	(0.006)	(0.008)	(0.002)	(0.002)
Intensity*Post*Public	0.0221***	0.0209***	0.0159***	0.0149***	-0.0016	-0.0014
	(0.004)	(0.004)	(0.005)	(0.005)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	880	880	880	880	880	880
R-squared	0.861	0.867	0.815	0.821	0.482	0.488

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table 1.5 reports the results of the impact of the intervention on demand side variables in the DDD framework. Columns (1) and (2) of Panel A show that the policy has significantly boosted the number 6th graders for both public and private schools in areas with low initial enrollment rates. In fact, a one-percentage point increase in the pre-reform prefectural non-enrollment rate is associated with a rise in the 6th grade access by nearly 2% for public schools per prefecture per year. The impact on private schools is also positive and significant but vanishes after controlling for some supply side variables. In any case, this is a positive result since private schools also gained about 0.9% to 1.5% final graders compared to the pre-policy level. This means that a prefecture that records a 10-percentage point rise in exposure to the policy would experience on average a 20%-rise in the number of 6th graders in its public schools and 9% to 15% in its private schools. Panel B and C report a 0.4-percentage point higher increase for public school girls than their male counterparts. In fact, about 2.2% more girls access the final grade in low pre-reform enrollment areas compared to roughly 1.8%-increase for boys. This suggests a narrowing of the gap in final grade attainment between boys and girls across prefectures as result of the FPEP. Since low enrollment localities are also in the poorest regions and given that there is merely no difference between private school boys and girls, this finding confirms the hypothesis that budget-constrained households would have preference over educating their boys, and that the enactment of the policy would help girls catch up on their male counterparts.

Columns (3) and (4) report about 1.3 to 1.4% increase in the count of public school graduates in prefectures with high baseline non-enrollment rates. The outcome is also positive for private schools though not statistically significant. Once again, this is an interesting result as the reform has not led to a decline in the number of private school graduates in different pre-reform enrollment areas. Panel B and C suggest a 0.2-percentage point higher increase for public school

girls than their male fellows suggesting that the FPEP contributes to narrow the graduation gap between girls and boys. A 10-percentage point increase in exposure to the reform leads to 15-16% more girls graduating from public school subsequent to the intervention compared to 13-14% for boys.

However, the program has no significant impact on the graduation rate in both public and private schools across local areas. This means that the number of graduates is increasing at nearly the same pace as the size of the test taking cohort. This result suggests that the UPEP has not led to a deterioration of public education quality. A tentative explanation to this result may be found in the reaction of the supply of schools and teachers consecutive to the policy. If schools or classrooms are not built in low pre-policy enrollment areas, classrooms in existing schools may be overcrowded leading to adverse effects such as grade repetition and dropouts. Table 1.6 reports the estimates of the effects of the intervention on supply of schools, classrooms, and teachers across low and high baseline enrollment prefectures.

Table 1.6. Effect on Supply Across Prefectures and School Type

Panel A	**Log (Schools)**	**Log (Classrooms)**	**Log (Teachers)**
Intensity*Post*Private	0.0023	0.0032	0.0063
	(0.005)	(0.004)	(0.004)
Intensity*Post*Public	0.0160***	0.0148***	0.0145***
	(0.003)	(0.003)	(0.003)
Observations	880	880	880
R-squared	0.829	0.838	0.830
Panel B	**School/student**	**Classroom/student**	**Teacher/student**
Intensity*Post*Private	-0.0001	-0.0002	0.0001
	(0.000)	(0.000)	(0.000)
Intensity*Post*Public	-0.0000	-0.0001*	-0.0001**
	(0.000)	(0.000)	(0.000)
Observations	880	880	880
R-squared	0.659	0.518	0.225
Pre-reform mean	0.006	0.028	0.027

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Significance levels: *** = 1%; ** = 5%; * = 10%.

It is worth noting that public schools opened in low enrollment areas at a merely similar pace as the rise in final grade enrollment. In fact, the first column of Panel A reports a 1.6% increase in the number of public schools in prefectures with 1 percentage point lower enrollment rates. Similar impacts are estimated for public school classrooms openings and teachers hiring which rose by 1.5% in prefectures with 1 percentage point higher exposure to the reform. Interestingly, private schools were also positively affected though not in a statistically significant way. These results suggest that on average private school supply variables (school openings and

teacher hiring) have not been distorted subsequent to the government's intervention on the schooling market. The ratio number of schools per student has not been significantly affected suggesting that school openings consistently increased with the rise in enrollment in low pre-policy enrollment localities. However, the teacher-to-student ratio slightly declined in public schools without hurting student outcomes. In sum, public schools and public school teachers entered the market in prefectures with high pre-treatment non-enrollment rates.

1.8. Conclusion and Policy Implications

This paper provides one of the first causal evidence of the impact of universal primary education policies in Africa. The investigation is both important and contemporary since some countries are still embarking in this adventure at the expenses of non-negligible sources of public revenue. As such, this study makes a major methodological contribution to the literature on the topic and provides in-depth analyses of primary completion and graduation outcomes and subsequent supply side variations compared to earlier works on Eastern African countries.

I find that the intervention has substantially spurred public school outcomes relatively to private schools. In fact, the program has significantly increased access to the final grade of primary school as well as the count of graduates, leaving unaffected the graduation rate. The policy has remarkably boosted the number of final graders and graduates, more so in prefectures with low baseline enrollment rates. This suggests a narrowing of the gap between children from different wealth backgrounds in terms of primary school attainment and graduation. The effects are slightly higher for girls, suggesting a waning of the gap between boys and girls. There is no significant change in graduation rates, suggesting that the intervention has not deteriorated public education quality. Plausible explanations may be found in school openings and teachers hiring which

increased consistently with the surge in enrollment, more so in intensely treated areas. I also find that school districts with growing supply of schools and teachers experienced higher number of 6th graders and graduates with, once again, no significant impact on the graduation rate. It also appears that on average the intervention has not distorted private school outcomes. Conversely to some countries where the free primary education policy led to the deterioration of education quality in public schools, in Togo, public school openings and teachers hiring grew consistently with the rise in enrollment, annihilating the negative effect of overpopulated classrooms. Based on the positive results from both the demand and supply points of view, it can be concluded that the FPEP in Togo was an effective and useful tool to reduce disparities in primary school access, completion, and graduation between children from disadvantaged backgrounds and their counterparts from relatively wealthier families.

The findings of this study suggest that a free primary education policy is likely an effective tool to reduce the gaps in school attainment and completion between advantaged and disadvantaged populations. Such an intervention is more likely to yield significant welfare-enhancing results if ad hoc accompanying measures namely school openings and teachers hiring are taken to counteract the potentially negative effect of classrooms' overpopulation as result of school fees suppression. Therefore, other poor countries envisaging to implement such reforms would have to couple them with school and classroom constructions and recruitment of instructors. They also need to make sure that areas with initially low levels of enrollment have sufficient infrastructures to draw full benefits from the reform. In order not to crowd out private school supply outcomes, this government intervention on the school market may need to include a private school financing component designed to incentivize relatively low-cost private schools. In fact, these schools may either have to accommodate students leaving public schools in anticipation of

classrooms being overcrowded or may lose some of their students and subsequently reduce their supply.

Chapter 2. Does Free Primary Schooling Curb Child Labor? Evidence from Togo

2.1. Introduction

Child labor is a major socioeconomic issue facing the developing world in general and Sub-Saharan African countries in particular. In order to mitigate the economic vulnerability associated with poverty, some households send their children to work. Child labor can interfere with schooling and compromise children's educational achievements as well as their physical, mental, and social development, thus jeopardizing their future and their country's development. In conformity with the standards of the International Labor Organization (ILO), the laws in Togo prohibit child labor before the age of 15 years. However, due to their precarious socioeconomic conditions, some households allow or request their children to work on family-based farms or businesses, to perform street vending, or to work in rock quarries, in mines, in manufacturing plants, on building construction sites, etc. Some, especially girls, work as domestic servants, risking physical abuse among other issues. Children also engage in prostitution and others become victim of child-trafficking. Child labor is prevalent in many African countries especially in Togo where one child out of three engages in non-remunerated as well as economic activities for cash, in-kind, or non-monetary compensation. In fact, according to the 2007 and 2008 country reports of the U.S. Department of Labor (USDOL) on the state of child labor in Togo, about 32.7%[2] of children aged between 5 to 14 years old were working in 2006.

[2] This rate is computed according to the UNICEF definition of child labor and does not account for sampling weight. The weighted average is about 31.5%.

Some earlier works on the determinants of child labor have stressed the importance of education costs in explaining children's unlawful participation to the labor force as these costs limit children's school access (Canagarajah and Nielsen, 1999). Therefore, reductions in education costs and increased school access have been advocated as policy instruments to curb child labor. In this vein, in the 2010 report, the USDOL evoked the free primary education policy among the laws and regulations that would sustainably reduce the magnitude of the phenomenon. However, from a theoretical standpoint, it is not clear how such interventions affect child labor since school attendance and work are not mutually exclusive and thus children can adjust leisure as result of a change in the relative cost of education or in the household income (Ravallion & Wodon, 2000; Cigno & Rosati, 2005).

Given the extent of children's labor participation before the enactment of the universal primary education policy, it is crucial to evaluate the effect of such a large-scale pro-poor reform on restraining this practice. Two earlier works have examined the effect of the BRIGHT School Construction Program in Burkina Faso on children's propensity to engage in household chores and economic activities. The Program included two main components. First, a school was built in each of the intervention villages. Second, the project provided direct incentives in the form of school kits, textbooks, and school meals for all students, and take-home rations for girls with a monthly attendance rate of 90% or higher to encourage school attendance. Analyzing the effect of the Program, Kazianga et al. (2013) found that the intervention has significantly reduced children's household chores while De Hoop and Rosati (2013) showed that it rather increased children's participation in economic activities and household chores. The discrepancy in these two studies is mainly due to differences in specifying their regression discontinuity models. Therefore, from an empirical standpoint, it is also unclear how such interventions affect child labor. Moreover, little

is known on the impact of larger scale pro-poor education policies on child labor. This paper purports to inform the literature on the causal effects of such a nationwide suppression of public primary school fees on child work by addressing the following question: what is the impact of the free public primary education policy in Togo on child labor?

To address this question, this study uses data from the 2006 and 2010 waves of UNICEF's Multiple Indicator Cluster Survey (MICS). I compare, in a difference-in-differences framework, changes in child labor outcomes across intended beneficiaries of the policy and non-beneficiaries before and after the policy to identify the effect of the policy on child labor. I find that the free public primary education reform has led to 4-5 percentage point decline in overall child labor. An investigation of the different components of child labor reveals that the fall in overall child labor is mainly driven by the decline in children's likelihood to perform household chores and out-of-household activities.

The remainder of this paper is organized as follows. The next section presents a simple theoretical framework to apprehend a child's schooling and work decisions. Section 2.3 succinctly describes and summarizes the data. Section 2.4 discusses the empirical methodology. Section 2.5 presents the results while the last section concludes.

2.2. Theoretical Framework

This section relies on a standard utility maximization model to provide some insights into the relationship between a child's schooling and work decisions. Assume a unitary household with parents maximizing a utility function defined over household consumption, children's leisure, and children's schooling decision. In general, if a child attends school, a fixed amount of their time has to be allocated to attending classes and commuting to and from school. The budget constraint is

therefore not strictly convex as school attendance requires a fixed amount of time. The model relies on the following assumptions. The number of children in the household is assumed exogenous and set to unity and adult labor supply is fixed. Households do not have access to perfect capital markets. As such, human capital investment decisions are not separable from consumption decisions. Denoting by C the household's consumption, Y the household's income, H the hours of child work, w the child's hourly wage, L the child's leisure time, μ the monetary cost of education (tuition, book fees, etc.), τ the time spent attending and commuting to and from school, and S a dummy variable equal to 1 if the child is enrolled in school, the household's utility maximization problem can be written as follows:

$$\max_{C,L,S} U(C,L,S) \;\; s.t \begin{cases} C = Y + wH - \mu S \\ H + L + \tau S = 1 \\ S \in \{0,1\} \end{cases}$$

The fixed amount of time required by school attendance introduces non-convexity in the child's time constraint. Expressing C and L as a function of H and substituting these expressions in the utility function, the problem becomes:

$$\max_{H}(U_0^*, U_1^*) = max \begin{cases} U_0^* = \max\limits_{H} \; U(Y + wH, 1 - H, 0) & if \; S = 0 \\ U_1^* = \max\limits_{H} \; U(Y + wH - \mu, 1 - H - \tau, 1) & if \; S = 1 \end{cases}$$

What is the expected change in child labor after the enactment of the free public primary education policy? If the household does not send its children to school, children's time is entirely available for leisure and work and the available exogenous income is Y. There are four possible equilibrium solutions. If households are better off not sending their children to school, then the children will either work or neither work nor attend school. However, the latter is less plausible since child labor also includes child household chores (performed beyond a certain number of

hours) and child work in family business. Conversely, if households prefer schooling their children, then the children can either attend school without working (lower consumption but more leisure) or work while attending school (higher consumption, but less leisure).

The FPEP does not affect the budget constraint for families whose children are not enrolled neither before nor after the policy but changes the budget constraint for children enrolled before and after the reform in two ways. First, the reform reduces, ceteris paribus, their monetary cost of education. Second, through eventual supply side responses such as school openings, the policy may reduce the fixed amount of time devoted to school attendance due to school availability within a closer vicinity. The decline in the monetary cost of education would induce an upward shift in the budget constraint of households who send their children to school both before and after the intervention. If the reduction in school attendance time is negligible, the upward shift in the budget line for children who are enrolled in school before and after the intervention may cause leisure to increase (and child work to fall) or remain the same (and child labor to remain the same). However, if both changes (reduction in monetary cost of education and decline in school attendance time cost) occur subsequent to the reform, they could have opposite effects on the likelihood of child work for children who are enrolled in school before and after the policy. In fact, the reduction in their school attendance time will increase the time available for work and leisure. The resulting change in child work is therefore ambiguous. Children who were combining work and school before the reform may continue or stop working afterward. Children who were in school only may or may not start to work.

I now consider children who were not in school and begin attending school as a result of the intervention. The rise in their monetary cost of education causes their family's budget constraint to shift downward. Moreover, the budget constraint is shortened as school attendance

time for these children increases. The household may try to react to this downward shift in the budget line by increasing the children's hours worked. However, there is now less time remaining for work and leisure. Hence, the effect of the FPEP on child labor is also undetermined for children not enrolled in school before the policy but enrolled afterward. Some of these children who were working before the reform may continue working or stop working. Others who were not working may even start to work. This simple representative agent utility maximization analysis suggests that the impact of the FPEP on child labor is undetermined.

2.3. Data and Summary Statistics

2.3.1. Data Description

The dataset consists of the 2006 and 2010 waves of the UNICEF's Multiple Indicator Cluster Survey (MICS). These are nationally representative stratified and clustered multi-households' survey data which provide information on households and their members' characteristics. The country is first divided in 12 strata obtained as follows: the 5 administrative regions plus the township of the capital Lomé are split into urban and rural areas. Then, a two-stage cluster survey is performed in each stratum independently from one stratum to the other. In the first stage, a sample of enumeration areas or primary units or clusters is drawn with probability proportional to their size in terms of population. The second step consists in drawing, via a simple random poll without replacement, a constant number of households or secondary units from each primary unit. A total of 6600 and 6975 households have been surveyed respectively in 2006 and 2010. This corresponds to a total of 10,018 children interviewed in 2006 and 9284 surveyed in 2010. Once a household is selected, a questionnaire is administered to each of its members. The survey collects among others information on demographic, educational, and socioeconomic

characteristics as well as information related to child labor. Regarding the latter, all 5- to 14-year-old children in the selected households are interviewed on their school enrollment and attendance status, household chores, out-of-household non-remunerated activities, economic activities carried out for the household or a non-member of the household in the week prior to the interview. According to the United Nations International Children's Emergency Fund (UNICEF), a child is considered working if (a) s/he is 5–11 years old who, during the reference week, did at least one hour of economic activity and/or more than 21 hours of unpaid household services, or (b) s/he is 12–14 years old who, during the reference week, did at least 14 hours of economic activity or more than 21 hours of unpaid household services.

2.3.2. Summary Statistics

This subsection presents some key features of the data. Table 2.1 displays means for the whole sample as well as for the sample split by year and intention-to-treat status. Potential beneficiaries are primary school children and non-enrolled primary school-aged (6-11 years old) children at the time of the survey. The 2006 questionnaire did not record the type of school (public or private) attended by a child. Non-beneficiaries are children in secondary school or those not enrolled in school. The first two columns report means and standard deviations for the entire sample, while the remaining columns show these statistics for potential beneficiaries and non-beneficiaries before and after the free primary education policy. A number of sociodemographic and socioeconomic characteristics have been identified in the literature as potential determinants of child labor. The dataset enables to account for some of these variables namely the wealth index score used as proxy for a family's wealth, the household size, the presence of a child's genitrix in the household, the mother's education level, the education level of the household head, and the

area of residency (urban or rural). The regression analyses model away over time observable differences between the two groups by including these characteristics as control variables.

Table 2.1. Means by Year and Treatment Status

	All years	2006			2010		
	All groups	All groups	Potential beneficiaries	Non-beneficiaries	All groups	Potential beneficiaries	Non-beneficiaries
Child labor	31.4	31.5	34.0	17.5	31.2	33.4	20.2
Rem. out-of-HH child labor	3.1	3.2	3.4	2.1	3.0	3.3	1.4
Non-rem. out-of-HH child labor	7.1	6.6	7.1	3.5	7.7	8.2	5.0
Child's HH chores	7.2	6.5	6.6	6.0	8.1	7.8	9.6
Child labor in a family business	20.1	20.8	23.0	8.8	19.3	21.8	6.9
Girl	49.5	50.1	50.6	46.9	48.8	49.0	47.9
Son/daughter of the HH head	75.1	74.0	73.2	78.4	76.3	76.9	73.3
Wealth index score	-0.134	-0.171	-0.213	0.062	-0.093	-0.171	0.299
HH size	7.321	7.383	7.415	7.204	7.253	7.270	7.167
Genitrix lives in the HH	74.2	73.3	72.4	78.1	75.2	76.2	70.6
Mother's educ. lev. = None	59.3	61.9	63.5	52.7	56.5	58.6	45.8
Mother's educ. lev. = Primary	28.2	26.8	26.4	29.0	29.8	29.5	31.3
Mother's educ. lev. = Secondary +	12.5	11.3	10.0	18.4	13.7	11.9	22.9
HH head's educ. lev. = None	43.4	45.8	47.6	35.9	40.8	42.5	32.1
HH head's educ. lev. = Primary	29.2	28.5	28.4	29.2	29.8	30.6	25.8
HH head's educ. lev. = Secondary +	27.4	25.6	23.9	35.0	29.4	26.9	42.1
Urban area	31.0	31.3	29.6	40.6	30.7	27.7	46.1
Number of observations	19302	10018	8568	1450	9284	7829	1455

Notes: All figures, except the wealth index and the household size, are in percentage. HH refers to household.

The table shows that 31.4% of children in the sample are engaged in child labor. Decomposing the child labor variable into its individual components reveals that 3% of children in the entire sample perform out-of-household remunerated works while about 7% perform unpaid out-of-household activities and 7.2% engage in household chores. It also appears that child labor

in family business is the most important form of child labor in Togo. Child labor is mainly prevalent in family business including working in one's family shop, farm, etc. or performing street vending for a family member. In fact, about 20% of children are engaged in this form of child labor. Therefore, any policy that is intended to curb child labor should aim at reducing substantially this form of child work.

A stratification by treatment status shows that 34% of the potential beneficiaries were working in 2006 compared to 33.4% in 2010, which represents about 0.6 percentage point decline over time for the intendedly treated children. Meanwhile, child labor for non-beneficiaries increased by 2.7 percentage point. A simple difference-in-differences on unconditional means leads to a 3.3 percentage point decline in child labor for intendedly treated children. There are no drastic changes in the share of potential beneficiaries and non-beneficiaries engaged in remunerated out-of-household child work before and after the policy. Conversely, the proportion of potential beneficiaries and non-beneficiaries involved in household chores increase respectively by about 1.2 and 3.6 percentage point. A naïve difference-in-differences on unconditional means suggests that the FPEP led to a 2.4 percentage point reduction in potential beneficiaries' propensity to perform household chores. As for child labor in family business, a stratification by treatment status reveals that about 23% of potential beneficiaries and 8.8% of non-beneficiaries were engaged in family business activities before the policy versus 21.8% of intended beneficiaries and 6.9% of non-beneficiaries after the policy.

Some child and household level characteristics can positively or negatively influence child labor. Failing to account for them may bias the estimates of the effect of the intervention on child labor. The first household level characteristic identified as potential determinant of child labor is the household's wealth. A principal component analysis was performed using information on

property ownership and household equipment (land and livestock not included) in order to assign weighting coefficients to assets of each household and obtain wealth scores for each household in the sample. The variables considered in the calculation of the wealth index are as follows: sources of water supply, type of sanitary facilities, wastewater disposal method, number of bedrooms in the house, nature of floor-wall-roof of the dwelling, type of energy source used for lighting and cooking food, and some possessed goods (electricity, television, telephone, car, refrigerator, cooking appliances, etc.). The average wealth index score for the whole sample is -0.133. In general, potential beneficiaries' households are less wealthy. The second household level potential determinant of child labor considered in this study is the household size. There are 7.3 people on average in a household. This statistic is consistent across the two subpopulations before and after the academic year 2008-2009. A third characteristic susceptible to affect child labor is the presence of the child's biological mother in the household. 74.2 % of children in the sample report that their genetrix lives in the household. In 2006, only 72.4% of potential beneficiaries were living with their biological mother compared to 78.1% for non-beneficiaries. In 2010, 76.2% of the potential beneficiaries were living with their genetrix while only 70.6% of non-beneficiaries had their mother present in the household. Other variables that could influence child labor are the mother's education level and the household head's education level. Most kids (59.3%) reported that their mothers have no education level, 28.2% report that their mothers have a primary school level, and 12.5% report a secondary or post-secondary education level. As for household heads, they seem more educated. In fact, 43.4% of kids reported that their household head has no education level, 29.2% reported a primary education level, and 27.4% reported a secondary or post-secondary education level. Finally, the area of residency can also affect child labor. About 31% of kids of the entire sample are living in urban areas. Before the policy, about 29.6% of the potential beneficiaries

versus 40.6% of non-beneficiaries were living in urban areas. Afterward, 27.7% of the intendedly treated children versus 46.1% of non-beneficiaries were living in urban areas.

2.4. Empirical Strategy

The policy was designed to improve access to primary school by eliminating primary education fees since they have been found to be an important constraint to school attainment[3]. To disentangle the effect of the reform on children's participation to the labor force, one approach would be to compare children that are 5-14 years to older teenagers in a difference-in-differences setting. Unfortunately, the questionnaire does not collect information on this latter group of children. Another identification strategy would have been to construct a synthetic control for Togo using neighboring countries. However, only 2 years of pre-policy data are available, rendering impossible the implementation of this methodology.

One way to overcome these constraints inherent to the data is to compare differences in child labor outcomes between treated (beneficiaries of the free primary education policy) and untreated children before and after the policy in a difference-in-differences approach to recover the treatment effect on the treated. However, the questionnaire did not provide any information on the type of school (public or private) attended by a child before the reform. Therefore, to overcome this other difficulty, I compare the intended beneficiaries of the free primary education policy to the non-beneficiaries before and after the intervention. Intended/potential beneficiaries are defined as children in primary school and non-enrolled primary school-aged (6-11 years old) children

[3] See Chapter 1 for references.

while non-beneficiaries are 5-year-old children not in school and secondary school students. Since there is no information on the type of school attended by a child before the enactment of the policy, this strategy would recover the intention-to-treat (ITT) effect rather than the treatment effect on the treated (TOT) per se. The TOT may be extrapolated if one assumes that the share of public primary school students in the sample is representative of the observed proportion in the population. The model can be written as follows:

$$y_{ihpt} = \beta_0 + \beta_1 Treat_{ihp} * Post_t + X_{ihpt}\alpha + W_{hpt}\gamma + \delta_p + \tau_t + \epsilon_{ihpt} \quad (2.1)$$

where y_{ihpt} denotes the child labor outcome for child i of household h of prefecture p in year t; $Treat_{ihp}$ is a dummy variable equal to 1 if child i of household h of prefecture p is in primary school and 0 otherwise; $Post_t$ is the post-policy year dummy; X_{ihpt} represents child's characteristics that can explain child labor; W_{hpt} denotes the aforementioned household characteristics likely to affect child labor; δ_p and τ_t represent respectively prefecture and year unobserved heterogeneities; ϵ_{ihpt} is the idiosyncratic error term. Note that $Post_t$ is not separately included in the model since it is perfectly collinear to the year fixed effects. Standard errors are clustered by prefecture to account for within-local area correlation in errors over time. Ideally, standard errors should be clustered at the enumeration area level. However, to preserve survey respondents' anonymity and data confidentiality, the identifiers of the enumeration areas have been suppressed from the databases.

The first individual characteristic controlled for is the child's sex. In fact, in Africa, girls are generally more involved in child work in the family context by performing household chores more than their boy siblings. Being a girl may therefore be associated with more child labor. A sociological factor accounted for is the child's kinship with the household's head. The biological

children of the household's head may be less likely to engage in child labor compared to any other child in the household with less strong family ties. Children entrusted by their parents to other families (a common practice in the country known as "*confiage*") may be more likely to engage in child labor than children living with their own parents. I therefore controlled for the fact that a child is the son or daughter of the household's head. Another similar potential determinant of child labor is the presence of the child's genitrix in the household. It may be the case that children living with their biological mothers are less prone to child labor than those whose genitrices do not dwell in the household. For example, children living under the custody of stepmothers may be more likely to engage in child labor than children living with their own mothers. Finally, the education level of a child's mother can influence a child's propensity to engage in child labor. More educated mothers may be less likely to allow this practice.

As for household level determinants, several hypotheses have been considered in the literature as potential reasons for child labor. One of these hypotheses, probably the main one, posits that poverty is the main driver of child labor. In fact, a child from a wealthy family is less likely to engage in child labor. However, rich families may exploit other people or relatives' children in their shops, businesses, farms, etc. Therefore, the effect of wealth on child labor may be ambiguous. The education level of the household's head can also affect child labor propensity. In fact, more educated household heads may be less likely to endorse child labor. The size of the household can also explain children's illegal participation to the labor force. Larger households may face more financial constraints and may therefore allow or require their children to perform remunerated works or work in family businesses to support the household's income. Finally, I controlled for the type (urban/rural) of area of residency. However, it is not clear how the area of residency affects child labor. While it is likely that children in rural areas can engage in farming

activities, it is nonetheless likely that children in urban areas can work in shops or shopping centers. The econometric model accounts for this by controlling not only for the type of residency but also for local area-specific unobserved determinants of child labor.

2.5. Results

This section presents and discusses the results of the impact of the free primary education policy on child labor. Since child labor is a binary outcome, probit regressions were performed along with linear probability model (LPM) to check the robustness of the results. In the same vein, each specification is modelled respectively without and with control variables. Table 2.2 below presents the results of the regressions. Since the magnitude of coefficient estimates obtained from probit regressions is not directly interpretable, the probit regression estimates shown throughout this chapter are the average marginal effects (AME) of each regressor.

Table 2.2. Effect on Overall Child Labor

	LPM		Probit	
	(1)	(2)	(3)	(4)
Treat*Post	-0.0377*	-0.0443**	-0.0449**	-0.0509**
	(0.020)	(0.021)	(0.023)	(0.023)
Treat	0.1618***	0.1459***	0.1623***	0.1488***
	(0.018)	(0.019)	(0.018)	(0.019)
Girl		0.0295**		0.0303**
		(0.012)		(0.012)
Son or daughter of the household head		-0.0059		-0.0073
		(0.013)		(0.014)
Genitrix lives in the household		-0.0239*		-0.0238**
		(0.012)		(0.012)
Mother's education level		0.0039		0.0040
		(0.009)		(0.009)
Education level of the household head		-0.0231***		-0.0233***
		(0.008)		(0.008)
Wealth index score		-0.0463***		-0.0491***
		(0.010)		(0.010)
Log(Size of the household)		0.0297*		0.0300*
		(0.017)		(0.017)
Rural area		0.0383*		0.0412**
		(0.019)		(0.020)
Observations	19096	19096	19096	19096

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include prefecture, and year fixed effects. The probit coefficient estimates reported are AME. Significance levels: *** = 1%; ** = 5%; * = 10%.

It is worth noting that the results obtained from the LPM are remarkably close to the AME derived from the probit regressions. The coefficient on the interaction term between the treatment dummy and the post-policy indicator is negative and statistically significant at the 5% level. This suggests that conditional on child and family characteristics, the free primary education policy led

to about 4 to 5-percentage point decline in child labor for intendedly treated children. As mentioned earlier, this is the intention-to-treat effect of the FPEP rather than the treatment effect on the treated since the pre-treatment survey did not identify the type of school (public or private) attended by each child. Though slightly lower, these estimates are of the same order of magnitude as those estimated by Kazianga et al. (2013) in their evaluation of the impact of the BRIGHT program on students' propensity to engage in some activities such as collecting firewood, house cleaning, fetching water, caring for siblings, tending animals, and help farming. They find that the BRIGHT program had a significant negative effect on child's household chores in Burkina Faso. However, the relatively moderate magnitude of the effect suggests that there are other important drivers of child labor than non-affordable school fees.

Hence, turning to the potential determinants accounted for, it appears that on average girls are a 3-percentage point more likely to be involved in child labor than boys. This confirms the previous hypothesis that girls may be more inclined to child labor because of their intensive involvement in household chores. Even though being a son or daughter of the household's head has the expected negative coefficient, it does not seem to be a statistically significant determinant of overall child labor. However, as expected, the presence of a child's biological mother in the household tends to have a significant deterrent effect on the child's unlawful participation to the labor force. In fact, children living with their genitrices are 2.4 percentage point less likely to work than their counterparts whose mothers do not live in the household. The mother's education level does not seem to be an important determinant of overall child labor as its coefficient is not statistically significant. However, as expected, the education level of the household head negatively and significantly influences child labor. In fact, an additional level of education of the household head is associated with a 2.3-percentage point decrease in the likelihood of child labor.

Children in households whose heads are more educated are 2.3 percentage point less likely to engage in child labor. Besides, the family's standard of living is a significant determinant of child labor. Children from wealthier families are less likely to be involved in child labor. A unit increase in the household wealth index score is associated with about 5-percentage point decline in a child's propensity to work. The number of people living in the household seems to be a non-negligible determinant of child labor. A 1% increase in the size of the household leads to a 3-percentage point drop in child labor at the critical level of 10%. Finally, the type of area of residence also appears to significantly affect child labor. Children living in rural areas are about 4-percentage point more likely to engage in child labor than their counterparts in urban areas.

To identify which type of child labor is driving these results, separate regressions were performed on each component of child labor, namely child's household chores, child labor in family business, remunerated and non-remunerated outside-household child work. Table 2.3 below shows the results of the probit regressions for each type of child labor.

Table 2.3. Effect on Each Type of Child Labor

	Child HH chores		Child Labor in Family Business		Rem. out-of-HH Child Labor		Non-Rem. out-of-HH Child Labor	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*Post	-0.0239*	-0.0211*	0.0184	0.0116	0.0118	0.0107	-0.0138	-0.0117
	(0.012)	(0.012)	(0.031)	(0.031)	(0.009)	(0.009)	(0.018)	(0.018)
Treat	0.0082	0.0014	0.1299***	0.1188***	0.0128***	0.0112***	0.0418***	0.0422***
	(0.01)	(0.012)	(0.011)	(0.013)	(0.004)	(0.004)	(0.008)	(0.008)
Girl		0.0494***		0.0047		-0.0099***		0.0006
		(0.005)		(0.011)		(0.002)		(0.005)
Son/daughter of the HH head		0.0054		-0.0079		0.0013		-0.0084
		(0.007)		(0.012)		(0.007)		(0.011)
Genitrix lives in the HH		-0.0320***		-0.0089		-0.0005		0.0039
		(0.007)		(0.013)		(0.005)		(0.009)
Mother's educ. lev.		0.0088		-0.0213*		0.0002		0.0113**
		(0.007)		(0.011)		(0.004)		(0.005)
Educ. lev. of the HH head		0.0009		-0.0171**		-0.0050*		-0.0061
		(0.005)		(0.007)		(0.003)		(0.005)
Wealth index score		0.01		0.0601***		-0.0114		-0.0123*
		(0.011)		(0.019)		(0.014)		(0.007)
Log(Size of the HH)		-0.0197***		-0.0437***		-0.0114*		0.0003
		(0.004)		(0.011)		(0.006)		(0.006)
Rural area		0.0081		0.0454***		-0.0017		-0.0107
		(0.006)		(0.012)		(0.006)		(0.011)
Observations	19057	19057	19096	19096	19057	19057	19057	19057

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include prefecture, and year fixed effects. AMEs are reported. Significance levels: *** = 1%; ** = 5%; * = 10%.

The first two columns of Table 2.3 show that the free public primary education policy led to about 2 percentage point decline in child's household chores at the 10% critical threshold. Columns (3) to (8) suggest that the policy has not significantly affected child labor in family business and out-of-household child labor. The policy has not affected children's propensity to engage in street vending on the account of the family or work in family shop, farm, or perform remunerated out-of-household works, etc. Children may be requested to perform these activities as they often represent a source of income for poor households. If child labor serves mainly as a

source of income for financially constrained households rather than being inherent to unaffordable primary school fees, children may continue to work even after school fees have been eliminated.

In sum, it appears that the decline in overall child labor is driven by the fall in children's engagement in non-economic activities. In fact, the first two columns of Table B.1 in Appendix B indicates that the free public primary education reform caused about 4-percentage point decline in child's non-economic activities. A stratification by gender reveals that the drop mainly occurred in the subpopulation of girls. The last two columns of Table B.1 echo this result. By allowing more girls, mainly involved in household chores, to enroll in school, the policy might have led to a reduction in the number of hours they spend on household chores. This is probably the case since these types of activities are performed on a daily basis, limiting the possibility of reallocating hours. However, this was not the case for economic activities. In fact, Table B.2 in Appendix B reveals that children's involvement in economic activities, are they performed for the family or outside the household, has not been affected by the FPEP. It is possible that these activities are not performed daily and, as such, hours can be reallocated in a way that children can still engage in such activities while attending school. Since these activities are generally sources of income for poor households, children likely continued to perform them even after the policy.

2.6. Conclusion

This study finds that the free public primary education policy enacted by Togo in the school year 2008-2009 led to about 4-5 percentage point decline in child labor for intended beneficiaries of the program. The actual treatment effect on the treated may be lower or higher. A decomposition of the overall indicator of child labor into its individual components reveal that this result is mainly driven by a decline in children, especially girls, participation to household chores and non-

remunerated out-of-household activities. The reform has no significant effect on a child's propensity to engage in economic activities (working in family business or performing out-of-household remunerated activities). This result is not surprising given the context of child labor in the country. In fact, by increasing school access, the policy may lead to a reduction in the number of hours children, particularly girls, devote daily to household chores. However, by reallocating hours, they may keep working in family business and performing remunerated out-of-household activities as these activities generally represent a source of income for their families. Curbing these types of child labor as well may require coupling the free primary education reform with pro-poor welfare-enhancing programs. This finding suggests that large-scale nationwide programs that reduce the monetary costs of education are not sufficient to reduce certain types of child labor even though they effectively increase school attendance. Therefore, free primary education policies implemented with the aim to achieve an increased school attainment and a reduction in child labor may need to be coupled with other interventions that can effectively reduce children's participation in economic activities.

Chapter 3. The Impact of Plastic Money Use on VAT Compliance: Evidence from EU Countries

3.1. Introduction

Over the recent decades, several countries around the world have introduced policies to limit cash use and incentivize the use of electronic means of payments in order to combat money laundering, terrorism financing, and tax evasion. Despite the emergence of new risks for tax compliance associated with technological change, the use of cash remains central in money laundering (Europol, 2015). Cashless payments can influence VAT evasion by limiting the ability of small businesses and the self-employed to maintain tax-evading, parallel cash activities for which cash revenue is a necessary requirement (Clearly Morse et al., 2009). From 2005 to date, an increasing number of European countries have introduced upper limits to cash payments. In total, 17 out of 28 EU member countries have implemented some sort of cash restriction as of September 2017. These cash restrictions range from EUR 500 in Greece to EUR 15,000 in Poland. In Spain, the government is contemplating to further reduce the current limit of cash payments (EUR 2,500 enacted in 2012 to EUR 1,000). The institutional response by the European Commission and the European Central Bank (ECB) to cash limiting legislations has been cautious. Recital 19 of Council Regulation (EC) No 974/98 on the introduction of the euro states that 'limitations on payments in notes and coins, established by Member States for public reasons, are not incompatible with the status of legal tender of euro banknotes and coins, provided that other lawful means for the settlement of monetary debts are available', a position maintained throughout the circulation of the euro as documented in ECB publications (e.g. ECB Opinions CON/2002/24, CON/2003/25, CON/2013/18, CON/2014/4, CON/2014/37, CON/2017/18, and CON/2020/17

among others). However, concerns have been raised that limitations on cash payments may constitute a breach of the legal tender status of cash. This has prompted recommendations for cash limit levels that are proportionate to the targeted money-laundering schemes.[4]

In some countries where these express cash restrictions are absent, the authorities require special reporting of cash payments. A marked example is the Form 8300 system in the United States which requires that any person engaged in a trade or business who receives more than $10,000 in one transaction or several related transactions must file a cash payment report to the Internal Revenue Service (IRS). This system is supposedly used by the government to track tax evaders and individuals profiting from criminal activities.

Given all these cases of explicit or tacit cash restriction policies, it is of paramount importance to evaluate whether the belief that limiting cash transactions reduces tax evasion through increased use of electronic means of payments holds empirically. Has the proliferation in credit and debit card usage or the use of other electronic forms of payments (plastic money, hereafter) contributed to increase compliance with the consumption tax in the European Union (EU)? The link between VAT compliance and plastic money is justified by the fact that plastic money is the primary substitute to cash, and because plastic money transactions are traceable by tax authorities, they are less prone to tax evasion than cash transactions. The question deserves attention since the shift in norms and preferences regarding forms of payments presents an excellent opportunity for tax administrations to close the consumption tax compliance gap either

[4] The *Consultation Strategy* of the European Commission on February 2, 2016 (European Commission, 2016) and the *Opinion* of the European Central Bank of 30 December 2019 (CON/2019/46) on limitations to cash payments can be accessed here: https://ec.europa.eu/info/sites/info/files/consultation_strategy_final.pdf and here: https://eur-lex.europa.eu/legal-content/EN/TXT/PDF/?uri=CELEX:52019AB0046&from=EN

directly by bringing previously unreported transactions into the tax net or by using newly generated third-party information (Kleven et al., 2011). Slemrod et al. (2017) investigate the effect of reporting requirements on sole proprietorship income obtained from electronic transactions such as credit card payments using confidential IRS tax return data. They estimate a small but robust increase in tax filing and reported tax receipts for a subset of taxpayers more prone to behavioral response. Between 2000 and 2016, the average share of transactions cleared via plastic money in the 26 EU countries increased from EUR 34.6 billion to EUR 111.9 billion. In terms of nominal GDP, this represents a growth from 9.3% to 19.5%. And, as noted by Auer et al. (2020), the COVID-19 lockdowns and rise in beliefs regarding viral transmission via cash may only compound the increasing trend toward plastic money use.[5] At the same time, tax administrations in the EU, indeed around the world, have implemented a number of innovations aiming to capture "hard-to-tax" tax bases, which would appear have improved overall tax compliance but which have also confounded the potential impact of the shift in consumer preference in favor of plastic money. Therefore, up to now it remains unclear how important the role of plastic money has been in narrowing the observed tax compliance gap in the EU. Obtaining an answer to this question would be helpful also to other countries, especially developing countries, struggling with tax enforcement issues.

The seminal paper on the importance of third-party information in the context of the VAT by Pomeranz (2015) establishes the first-order deterrence effect of the paper trail generated by transactions between firms in Chile. An empirical literature has emerged with works closer to the

[5] Albeit not relevant to the tax compliance focus of this paper, it is important to note that the documented rise in the digital payments could have adverse distributional effects on the unbanked and vulnerable populations (Auer et al. 2020).

research question of this study. First, Madzharova (2020) explores the impact of cash and card transactions on VAT collection efficiency in the European Union (EU) member states from 2000 to 2010 and finds a statistically significant positive effect of card payments on tax revenue performance. Second, Hondroyiannis and Papaoikonomou (2020) analyze the effect of card payments on VAT revenue in 19 European Monetary Union (EMU) countries using panel quarterly information from 2003 to 2016. These authors find that a larger share of card payments in private consumption expenditure increases VAT revenue in the EU as a whole. Finally, Immordino and Russo (2018a) find some evidence of a positive effect of credit and debit card payments on VAT compliance in the EU while also showing that ATM cash withdrawals increase VAT evasion.

The first contribution of this study relative to Madzharova (2020) and Hondroyiannis and Papaoikonomou (2020) lies in the use of a less broad measure of VAT gap that is much more closely related to tax compliance. First, the dependent variable or observed outcome in our analysis is the VAT compliance gap as opposed to VAT Revenue Ratio (VRR) and VAT revenue used in pre-existing literature. This is computed as the difference between expected and realized tax revenues and may be interpreted as foregone tax revenues net of parameters that determine expected tax revenues such as tax rates.[6] The VAT compliance gap is less susceptible to the inclusion or exclusion of observable factors that can be accounted for (manipulation bias) as well as unobservable confounding variation from other factors that influence revenue collection, such as changes in tax administration quality or enforcement capacity, which we may fail to capture fully (omitted variable bias), or partly (measurement error). The fact that the outcome in this study

[6] The estimated VAT gaps are defined as the difference between expected VAT revenues according to the law and actual VAT collections. This includes "foregone VAT revenue from to tax fraud, tax evasion and tax avoidance. Moreover, the tax gap also incorporates revenues losses from bankruptcies, financial insolvency and errors." (source: https://ec.europa.eu/taxation_customs/business/tax-cooperation-control/vat-gap_en)

is pre-determined by the reports of the Directorate General for Taxation and Customs Unions (DG TAXUD) safeguards against the arbitrary choice of explanatory variables that would introduce manipulation bias into our results. Furthermore, even observable factors such as exemptions and zero-rates cannot be easily accounted for when using aggregate (national) level data. In the prior literature, these correlations had been relegated to the error term, potentially leading to omitted variable bias issues, whereas in this study, they are captured by the outcome variable by definition. Even though Immordino and Russo (2018a) employ appropriate measures of VAT compliance gap, they estimate the effect of a single mean of payment at a time without accounting for variations in the other means of transactions. Furthermore, they do not control for public authorities' efforts toward reducing the gap, which may overstate their estimates. These potential causes of omitted variable bias could lead to overestimate plastic money's role in reducing VAT gaps. Finally, with the availability of more recent data, this paper extends the examination of plastic money's impact in the literature to a period during which it became much more prominent throughout the continent. From 2012 to 2016, the years for which this paper provides novel estimates, debit and credit card use increased by 43% (25.5% as a fraction of GDP).

This study exploits variation in time and space of credit and debit card usage across 26 EU member states from 2000-2016 using panel data and instrumental variable techniques to identify the effect of plastic money on VAT compliance. It finds that an increase in the share of plastic money used for consumption expenditures is associated with a significant decrease in the VAT gap. The policy implications of these results are twofold. First, they highlight the role that technological supply-side innovations such as the use of plastic money can play in assisting the compliance efforts of tax administrations around the world with limited capacity. Second, they

demonstrate the limitations of some forms of third-party reporting in improving consumption tax compliance.

The remainder of the paper is organized as follows. The next section succinctly reviews the broad literature on the determinants of VAT compliance as well as the specific literature on the effect of plastic money use on VAT compliance. Section 3.3 describes the data, provides some key summary statistics, and delineates the empirical strategy. Section 3.4 presents and discusses the results while the last section concludes.

3.2. Literature Review

Several studies have empirically examined the economic, social, and institutional determinants of VAT compliance. While some studies analyzed the determinants of VAT revenues, rather than measures of VAT non-compliance per se, one needs to be cautious in comparing the results of these two groups of studies since VAT revenues and the VAT Revenue Ratio (VRR) reflect not only the effect of non-compliance but also the impact of policy choices on tax structure such as exemptions, reduced rates on certain transactions, etc. This section first reviews studies on the determinants of VAT compliance gap to circumscribe the universe of potential control variables and then focus on papers that investigate the effect of card payments on VAT revenue or measures of VAT efficiency.

In a pioneering study, Agha and Haughton (1996) constructed an index of VAT compliance for a cross-section of 17 OECD countries in 1987 and regressed it on characteristics of the countries and their VAT rates. The index was set as the ratio of actual VAT to potential VAT, analogous to the VRR. They find that a higher VAT rate is associated with lower compliance, and

that compliance is considerably lower with multiple VAT rates. The study also suggests that VAT compliance increases the longer VAT has been introduced.

Studying the factors that determine VAT fraud in Italy from 1982 to 2001, Otranto, Pisano and Polidoro (2003) find that VAT evasion is positively affected by variables such as the GDP, the share of the fiscal burden, and the ratio of gross profits over value added. However, it is negatively associated with the number of taxpayers audited in the previous year by public authorities. Christie and Holzner (2006) examine data for 29 European countries over the period 2000-2003. Their analysis reveals that higher VAT rates reduce VAT compliance whereas greater judicial and legal effectiveness improve compliance.

Another seminal study devised to quantify and analyze the VAT gap was the one commissioned by the DG TAXUD of the European Commission and executed by Reckon (2009). Investigating the relationship between the estimated VAT compliance gap and some economic and institutional variables, the report reveals that VAT gaps were significantly higher in countries with higher perceived levels of corruption. However, the study does not find robust statistical evidence of an association between the compliance gap and economic variables such as the sectoral composition of the economy, the GDP per capita, the level of taxation (VAT standard rate and theoretical VAT liability as a proportion of GDP), etc. One methodological issue with the Reckon report is that it relies on a random effect estimator which assumes that unobserved country-specific differences that determine taxpayer compliance are random, and thus uncorrelated to the explanatory variables of interest. This approach may suffer from omitted variable bias and therefore unlikely to produce reliable estimates of the determinants of the VAT gap. Aware of this possible limitation, the recent study commissioned by the DG TAXUD and conducted by the Center for Social and Economic Research (CASE) used a fixed effect estimation technique to quantify and analyze the drivers of

the VAT compliance in European countries. The final report released in 2018 suggests that the productive structure of the economy affects the VAT compliance gap with the biggest effect due to the retail sector. This means that the larger the share of the retail sale sector in the economy, the larger is the gap. It also reveals a positive correlation between unemployment as a proxy of liquidity constraints and the level of the compliance gap. However, the report does not find a consistent effect of the scale of the tax administration on the VAT compliance gap. From the regressions which include subsets of the explanatory variables, the authors find that higher administration costs lead to lower compliance gaps. However, once all the explanatory variables are introduced in the regression analysis, the study finds a positive, U-shaped association between the scale of the tax administration and the compliance gap, suggesting that higher administration expenditures hinder tax compliance up to a certain threshold and then become productive. In the same vein, Lago-Peñas and Lago-Peñas (2010) examine the determinants of tax morale in a multilevel analysis in the EU. They find that tax morale is a function of individual and contextual level variables. They show that tax morale in European countries varies systematically, on the one hand, with socio-demographic characteristics (age, gender, education level, and religion), personal financial experiences (income levels and self-employment), political attitudes (satisfaction with democracy, trust in politicians, and measures to reduce differences in income), and on the other hand, with regional GDP and tax arrangements. Due to the close correlation between tax morale and VAT compliance, this paper includes predictors of tax morale in its empirical analysis given that tax morale is not available in annual frequency for all the countries in the study sample.

Turning to studies that examined the effect of card payments on VAT revenue or VAT efficiency measures, a few works closely related to this paper are those by Madzharova (2020), Hondroyiannis and Papaoikonomou (2017, 2020), and Immordino and Russo (2018a).

Hondroyiannis and Papaoikonomou (2017) analyze the effect of card payments on VAT revenue after Greek authorities imposed restrictions on cash withdrawals in July 2015. They find that a 1 percentage point increase in the share of credit card payments in private consumption results in approximately 1% increase in VAT revenue. Hondroyiannis and Papaoikonomou (2020) examine the same relationship using quarterly panel data for the 19 Euro area economies from 2003-2016 to find that an increasing share of credit card payments in private consumption expenditure improves VAT revenue in the EU. Moreover, they show that gains from increased credit card use are decreasing in baseline card use and revenue efficiency levels while increasing in the share of self-employed individuals in the labor force with the largest gains reported for Greece and Italy. Madzharova (2020) investigates the impact of cash and card transactions on VAT collection efficiency in the European Union member states from 2000 to 2010. She finds statistically significant improvements on tax revenue performance from increased use of card payments and that cash usage has a consistent negative effect on VAT efficiency. A potential concern with this study is the use of observed VAT revenue or (VRR) as the measure of tax revenue efficiency without controlling in their estimations for contemporaneous tax policy changes of importance such as the enforcement capacity of the tax administration. This situation can lead to omitted variable bias in the estimates reported in these papers. In fact, tax fraud and tax evasion account for only a fraction of the entire VAT gap. The remaining portion of the total gap is due to tax structure policy decisions - the legal gap - which varies widely across space and time. In a nutshell, by using improper measures of tax compliance efficiency, these studies are limited in tracing out the effect of plastic money on tax compliance since the policy gap varies widely within a country and across countries. In this respect, Madzharova (2020) acknowledges that, ideally, one should use the compliance ratio. That said, the present paper has the advantage of using VAT compliance

gaps computed through the joint effort of a dozen tax administrations and research institutions in for a study commissioned by the DG TAXUD in 2015 on the VAT gap in the EU member states. This alternative outcome is also preferred by Immordino and Russo (2018a), the most closely related study to this paper. Even though an appropriate measure of VAT compliance gap is used in it, evaluating the effect of one mean of payments (either cash or card) at a time while abstracting from the other transaction method can pose the problem of omitting a relevant regressor if the two types of transactions are strongly correlated. In fact, economic agents do not perform only card payments or only cash transactions. The majority of agents does both and both are present at the same time in the economy and are related (complements or substitutes). Hence, the effect of the use of one means of payments on the compliance gap should be conditional on the use of the other one. Moreover, the study does not account for *ex ante* efforts made by public authorities to reduce the VAT gap, which can cast doubt on its estimates. For example, stronger tax administrations may advocate in favor of severe cash restriction policies in order to curb the gap. This can lead, once again, to omitted variable bias issue in their analysis. Finally, this paper also provides an update of Immordino and Russo (2018a) by extending the panel by four more years during which plastic money use increased markedly.

3.3. Data and Empirical Analysis

This section first describes the data utilized in this study and presents some key summary statistics on VAT compliance gap and plastic money use in the EU. Then, it discusses the methodology employed to tease out the effect of plastic money use on the compliance gap.

3.3.1. Data

The data utilized in this study stem from several sources and cover the period 2000 to 2016. The empirical exercise focuses on the effect of proliferation in credit and debit card use on consumption tax compliance, namely VAT compliance. The measures of VAT gap utilized are the VAT gap in EUR, the VAT gap as share of GDP, and the VAT gap as share of Value-Added Total Tax Liability (VTTL), which proceed from the 2013 and 2018 final reports on the VAT gap in the European Union member states (TAXUD, 2018). These data are available for all the 26 countries from 2000 to 2016. The first key explanatory variable is the value of card transactions by all cards issued in the reporting country as a share of GDP. Card payments cover the period 2000-2016 for all the countries but Bulgaria, Luxembourg, and Slovakia for which they are available from 2001, and Spain where they stem from 2002. The second main regressor is the ratio of cash withdrawals to GDP. The GDP data proceed from Eurostat database, while card payments and cash transactions are drawn from the European Central Bank (ECB) data warehouse. The analysis uses ATM cash withdrawals (pertaining to cards issued in the reporting country) which have fewer missing observations to approximate cash transactions.[7] In lieu of cash transactions, the analysis uses in some specifications the relative size of the shadow economy in the GDP as computed by Medina and Schneider (2018). The ECB also provides information on the number of ATM per million inhabitants.

Expected efforts made by the tax administration to close the VAT gap *ex ante* can also have a deterrent effect on private agents' tax compliance behavior. These efforts can be driven by the

[7] A more precise proxy for cash transactions would be the sum of the value of Automatic Teller Machine (ATM), Over-The-Counter (OTC), and Point-Of-Sale (POS) cash withdrawals. However, there are a lot of missing values on these variables for each country at different points in time. Hence, the total cash transactions constructed as the sum of ATM, OTC, and POS withdrawals may not be a good proxy for cash transactions.

scale of the tax administration (measured as the ratio of total administrative costs as a percent of GDP), the share of information and technology expenditures in total administrative costs, as well as the level of public indebtedness measured by the ratio of public deficit to GDP. The study therefore accounts for these 3 variables to proxy the effect of the tax administration on voluntary tax compliance, controlling for them in a lagged form to reflect expectations. The data on the scale of the tax administration and IT expenditures stem from the OECD data bank while the ratio of public deficit to GDP comes from Eurostat.

One factor supposed to shape tax morale, and thus potentially tax compliance, is age structure. In fact, the literature on tax morale finds that older people exhibit higher tax morale, which is commonly explained as being more aware of the benefits of adopting a prosocial behavior (Koumpias et al., 2020). The variable used to proxy tax morale is the share of people over 50 years old in the population obtained from Eurostat database. In addition, the less effective use of public resources by the government, the perception of private economic agents about the poor performance of public authorities, and liquidity constraints can create incentives to free ride and avoid tax payment (Godin and Hindriks, 2015). To account for the quality of government, this paper utilizes the World Bank's government effectiveness indicator. Besides, it controlled for the unemployment rate obtained from Eurostat to proxy for liquidity constraints and the business cycle. The analysis also accounted for over time cross-country differences in payment habits that depend on the level of economic development by controlling for the GDP per capita. Tax evasion can also depend on the productive structure of the economy in the sense that some sectors (like wholesale and retail trade) may be more prone to tax evasion than others (such as real estate). Therefore, the study controlled for the percentage share of the following sectors in the gross value-added (VA): wholesale and retail trade, transport, accommodation, and food service activities

(probably the key sector subject to tax evasion), as well as real estate, construction, industry, information and communication, and art and entertainment. Sectors that are exempted from VAT, namely health, education, or financial services, are excluded. These series as well as those on GDP per capita and population size are obtained from the Eurostat data bank. Finally, private agents' tax compliance behavior can be affected by prevailing VAT rates. However, VAT rates do not change very often. Hence, their impact on the compliance gap would be subsumed in country-specific heterogeneities. Following the 2018 TAXUD report, the paper controlled instead for the dispersion of tax rates (within a country), which provide incentives and opportunities for evasion, and the information for which was generously provided by the authors of the 2018 TAXUD report. Some of the control variables also record missing values for some countries in specific years. Given all that, the dataset is an unbalanced panel covering the period 2000-2016. Table C.1 in Appendix C provides summary statistics and data sources for all variables employed.

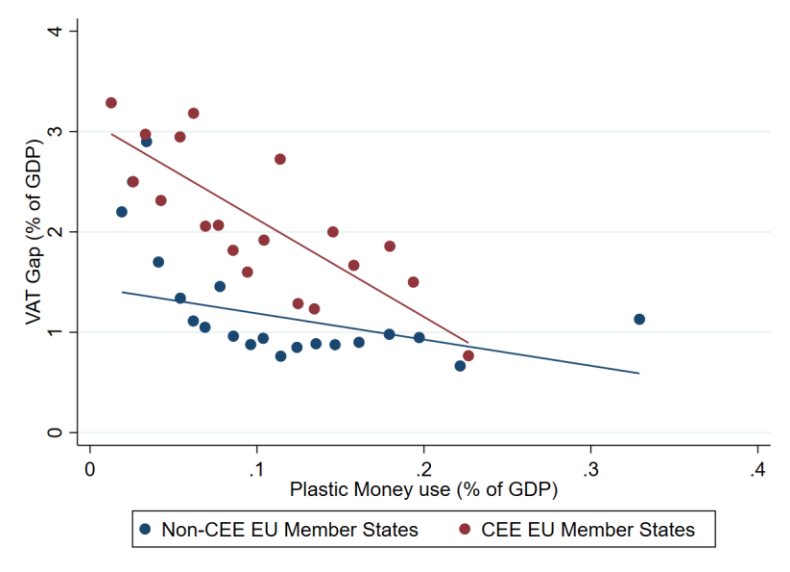
3.3.2. Descriptive Statistics

Figure 3.1 below is a binned scatterplot of the VAT gaps and plastic money use between 2000 and 2016. For each country studied, it plots the estimated average VAT gap in each plastic money usage bin (20 bins specified).[8] It is used to succinctly illustrate the relationship between VAT gap levels and plastic money use. Despite substantial heterogeneity, it provides first evidence of a negative correlation between VAT gaps and plastic money use. This association may also be the consequence of third factors that jointly impacted VAT gaps and plastic money use. Nevertheless, it highlights the fact that the proliferation of plastic money coincided with a

[8] For all countries but Bulgaria, Luxembourg, Slovakia, and Spain, data on plastic money use are available from 2000. For Bulgaria, Luxembourg, and Slovakia, these data stem from 2001. For Spain, they start from 2002.

reduction in VAT gaps. Furthermore, to highlight the differential relationship between plastic money use and VAT gaps across different blocs of EU countries, the sample was split by whether or not a country is classified as a Central and Eastern European (CEE) country according to the OECD. This illustrates that this inverse relationship of interest is relatively more pronounced for CEE countries which may be attributed to lower baseline plastic money usage levels in these countries.

Figure 3.1. VAT Gaps and Plastic Money Use in CEE and Non-CEE Countries



Notes: Binned scatterplot of VAT gaps and plastic money use by Central Eastern European (CEE) and non-CEE EU member states

3.3.3. Empirical Strategy

To identify the effect of plastic money on VAT compliance, this study exploits spatiotemporal variations in plastic money usage over time across different EU member states in a fixed effect (FE) panel regression framework. An advantage of the FE estimation technique is that it allows country-specific time-invariant unobservables to be correlated with the explanatory variables. The econometric model can be specified as follows:

$$Y_{it} = \beta_0 + \beta_1 W_{1it} + \beta_2 W_{2it} + \boldsymbol{X}_{it}\boldsymbol{\alpha} + \mu_i + \tau_t + \epsilon_{it} \quad (3.1)$$

where i denotes the country, and t represents the year; Y_{it} represents the outcome (VAT gap, VAT gap as share of GDP, and VAT gap as share of VTTL); W_{1it} is the main explanatory variable, defined as the logged total value of residents' card payments as share of GDP; W_{2it} represents the logged ATM cash withdrawals as share of GDP; $\boldsymbol{X}_{it}$ denotes the set of control variables (incentives created by the tax administration, government effectiveness, productive structure of the economy, tax morale, GDP per capita) discussed above in subsection 3.3.1. These regressors were included as they have been pre-identified as determinants of VAT gaps in the literature (TAXUD, 2018). Some control variables, namely cash withdrawals as share of GDP, population and GDP per capita, enter the model in their logarithm form. The term μ_i captures time-invariant country-specific heterogeneities that affect tax compliance while τ_t accounts for year-specific effects such as macroeconomic factors that affect indifferently tax compliance in the European economies under study; ϵ_{it} represents the idiosyncratic disturbance while β_1 and β_2 identify respectively the effect of plastic money use and cash transactions on tax compliance. Standard errors are clustered at the country level to account for serial within-country correlation in the data.

A potential concern with the model specified by equation (3.1) is that the choice to use a specific means of transactions may be endogenous to tax evasion. Immordino and Russo (2018b) propose a bargaining model where buyers may reach an agreement with sellers to engage in collaborative tax evasion for a reduced transaction price. To overcome this potential threat to identification, the analysis resorts to an instrumental variable (IV) approach. For the IV technique to be valid, a number of tests needs to be performed. First, one needs to test if the instruments are exogenous, i.e., if they are uncorrelated with the error process in the structural equation. In the present context, this implies that each of these instruments must affect plastic money use without itself being influenced by the VAT gap. Moreover, each instrument must affect the VAT gap only

through its effect on the payment method. A rejection of the null hypothesis of the Hansen J statistic of overidentification test casts doubt on the validity of the instruments. Second, one needs to check if the instruments are relevant, i.e., if they are sufficiently strong in predicting the endogenous regressors. This is obtained through the test of weak identification which compares a Wald-type F statistic to critical values tabulated by Stock-Yogo (2005). A rejection of the null hypothesis suggests that the model is not weakly identified. Since the specification allows for heteroskedastic errors, the Cragg-Donald Wald F statistic is no longer valid, and one needs to use instead the Kleibergen-Paap Wald rank F statistic. This test is important because in the presence of weak instruments, the loss of precision is severe, and IV estimates may even perform poorly than those obtained via Ordinary Least Squares (OLS). Hence, third, it is important to assess whether the potentially endogenous regressors should be treated as exogenous given the instruments. Under homoskedasticity, this is equivalent to the Hausman test of IV estimates versus OLS estimates.

Following Immordino and Russo (2018a), card and cash payments were instrumented by the number of ATM per million inhabitants and the number of fixed broadband internet connections per inhabitant. In addition, the number of bank branches per 100 km^2 (bank branch density) was employed as a third instrumental variable. The choice of the number of ATMs as instrument is justified by the fact that the availability of ATM terminals can stimulate cash withdrawals. It is likely that less direct card transactions are recorded where there are more ATMs available since it is easier to get cash. Besides, it has been documented that ATM development is mainly due to technological advances that curb their installation and management costs, ruling out the possibility that the diffusion of ATMs is a response to tax evasion. Finally, one cannot conceive any direct effect of ATM prevalence on tax compliance other than through its facilitation of cash

withdrawals (Humphrey et al., 2006). Therefore, the number of ATMs per million inhabitants is expected to be negatively and positively associated with card payments and ATM cash withdrawals, respectively, as they reflect individual demand for cash. Bank branch density was used to decompose the differential supply- and demand-side effects of access to banking services. Closely related in nature as another proxy of banking services availability, it is reasonable to assume bank branch density to be also positively associated with demand for cash and, subsequently, ATM cash withdrawals. The paper posits that bank branch density is additionally influenced by supply-side effects in the form of banking industry reorganization with the advent of e-banking, not VAT gaps. Thus, bank branch density is expected to be also positively associated with the share of card payments as a percent of the GDP. Similar arguments hold for broadband internet connections. The use of broadband internet connections as an instrument is motivated by the fact it facilitates electronic transactions which require cashless payments. Therefore, more broadband connections would induce more transactions cleared via plastic money and less demand of cash for transactions. Consequently, a positive and negative relationship between plastic money usage and cash usage, respectively, is expected. As in the case of ATM development, broadband connections mainly hinge on the availability of the infrastructure in a given country, which, in turn, is likely not correlated with VAT evasion. Further, the exclusion restriction requires that any effect of variations in broadband internet connections affects VAT gaps solely via plastic money use. This is a tenable assumption since more broadband internet connections directly affect VAT gap by requiring cashless transactions whereas any effect on tax enforcement is fully captured in the calculation of the VAT gap, by design (Immordino and Russo, 2018a). As stated earlier, the main difference with Immordino and Russo (2018a) lies in the fact that these authors considered one form of payments at a time, yet both can be endogenous. Hence, in the first stage, both the

logged card payments and cash withdrawals as share of GDP are regressed on the set of instruments and control variables which include country and year unobserved heterogeneities. In order to test for overidentification, the analysis includes the interaction term of the number of ATMs per million inhabitants with the broadband internet connection and the interaction term of the number of ATMs per million inhabitants with bank branches. The first interaction term is justified by the fact that card payments are likely determined by the combination of the two means, whereas the second interaction term accounts for non-commercial ATM attached to a bank branch as opposed to standalone ATMs that are more susceptible to variations in demand for cash. This intuition is confirmed by the results of the first stage IV regression reported in Table C.3 in Appendix C.

3.4. Results

The results indicate a strong correlation between card payments and cash withdrawals. Therefore, as stressed earlier, the concern regarding overestimation of the impact of plastic money use due to omitted variable bias in prior studies is valid. The analysis then examines a regression model that accounts for both means of transactions as well as tax enforcement endeavors of public administrations. Table 3.1 displays the results of the cross-country fixed effect panel regressions. Three different variations of the outcome are considered. In columns (1)-(2), (3)-(4), and (5)-(6), the dependent variables are respectively the logarithm of the VAT gap, the VAT gap as percent of GDP and the VAT gap as percent of VTTL. The preferred outcome variable is the VAT gap as share of GDP since one can similarly normalize the key independent variable by GDP. Columns (2), (4), and (6) report the estimates of regressions that include the size of the shadow economy rather than ATM cash withdrawals. Table C.2 in Appendix C shows the regression results when

an indicator for CEE countries and its interaction with card payment transactions are included as additional explanatory variables.

Table 3.1. FE Panel Regression of VAT Gap on Plastic Money Use

	Log (VAT gap)		VAT gap as % of GDP		VAT gap as % of VTTL	
Log(Card payments as % of GDP)	0.0482	-0.0306	0.1030	0.0736	0.5584	-0.1474
	(0.137)	(0.087)	(0.155)	(0.150)	(1.566)	(1.631)
Log(ATM cash withdrawals as % of GDP)	-0.0887		-0.1500		-2.5835	
	(0.224)		(0.140)		(1.654)	
Size of the shadow economy		-0.0976**		-0.0187		-0.2522
		(0.045)		(0.040)		(0.489)
Share of whole and retail sale in VA (%)	-0.0148	-0.0316	-0.0562	-0.0567	0.0526	-0.0503
	(0.030)	(0.025)	(0.056)	(0.049)	(0.391)	(0.332)
Share of real estate in VA (%)	-0.0614	-0.0478	-0.0195	-0.0128	-0.1995	-0.1785
	(0.055)	(0.048)	(0.056)	(0.060)	(0.510)	(0.554)
Share of arts in VA (%)	-0.0754	-0.0394	0.0347	0.0520	0.0372	0.1799
	(0.064)	(0.050)	(0.055)	(0.054)	(0.530)	(0.494)
Dispersion of tax rates	-0.7873	-1.9653	2.5325	1.6008	26.1645	14.9402
	(1.814)	(2.005)	(1.940)	(2.223)	(18.965)	(22.419)
Unemployment rate	0.0241	0.0423**	0.0554**	0.0512*	0.4771**	0.4600**
	(0.014)	(0.018)	(0.023)	(0.025)	(0.206)	(0.211)
Age structure	0.0750	0.0486	0.1367**	0.0949*	1.0273*	0.6665
	(0.051)	(0.044)	(0.059)	(0.054)	(0.561)	(0.492)
Deficit to GDP ratio	-0.0297	-0.0220	-0.0323*	-0.0292	-0.3296*	-0.2784
	(0.019)	(0.017)	(0.018)	(0.019)	(0.176)	(0.174)
Log(GDP per capita)	0.9493*	0.7085			1.8887	0.4060
	(0.463)	(0.502)			(3.817)	(5.200)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	412	411	412	411	412	411
R-squared	0.317	0.363	0.314	0.264	0.286	0.227
Number of countries	26	26	26	26	26	26

Notes: The following regressors are omitted from presentation: share of construction in VA, share of industry in VA, share of telecommunications in VA, log(population) and the constant term. Robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The OLS estimates reported in Table 3.1 suggest that plastic money use does not significantly affect VAT compliance in the EU. This result is consistent with the OLS results of Immordino and Russo (2018a) who find no impact of card payments on VAT collection efficiency. However, this result is not in line with Hondroyiannis and Papaoikonomou (2020) who find that the share of card payments in private consumption expenditure improves VAT tax compliance in the 19 Euro area economies. Next, it appears that the unemployment rate has a large, positive, and very precise association with all measures of VAT gap. Again, this is not surprising given that the unemployment rate is directly related to the size of the shadow economy which has been typically found to increase tax evasion and thus the tax compliance gap. Lastly, the naive OLS estimates suggest a positive and weakly statistically significant relationship between the GDP per capita and the level VAT gap. However, this is not confirmed when using the VAT gap as share of VTTL. It is not immediately obvious why; a potential explanation might be a common stochastic trend shared by these two macroeconomic aggregate time series.

To tackle the aforementioned potential endogeneity issue, the study now resorts to an IV approach using the prevalence of ATM, broadband internet connections, and geographical density of bank branches in a country as well as the interaction term of the number of ATMs per million inhabitants with each of the other instruments. Table C.3 in Appendix C reports the estimates of the first stage regressions.

The population density of ATMs and, to a lesser extent, the geographical density of bank branches are also predictive of card use which could indicate banking services availability. As expected, broadband internet connections, as an indicator of a digital society, are positively linked to plastic money usage and negatively linked to cash withdrawals. However, as ATM density (broadband internet connections) increases, the positive impact of broadband (ATM density) on card use

becomes less pronounced. Conversely, the negative effect of broadband internet connections on cash withdrawals is weaker when more ATMs per million inhabitants are available. Not surprisingly, ATM and bank branch density are positively related to cash withdrawals. The relatively stronger association between ATM density and cash withdrawals confirms our conjecture that ATM density reflects demand for cash whereas bank branch density may be also driven by supply-side changes in the banking industry. As more bank branches (ATMs) are introduced though, the effect of ATM (bank branch) density on cash withdrawals becomes negative, and vice versa. Still, the magnitude of the estimated negative effect of the interaction term is relatively smaller than the positive effects estimated for each of their factors. Thus, this small effect could reflect market saturation and the adverse influence of increasing banking services on cash withdrawals that does not reverse the main effects. Finally, a null effect of branch density (ATM density) on card use is recovered when ATM density (branch density) rises.

The Hansen J test of overidentification fails to reject the null hypothesis of overidentification of the models in all the three columns at the conventional critical level of 5%. This suggests that the instruments are valid. Moreover, the Kleibergen-Paap Wald rank F statistics compared to the Stock-Yogo weak identification test critical values suggest that the instruments are relatively strong. Finally, the endogeneity tests conditional on the instruments reject in all the cases the null hypothesis of exogeneity of plastic money use and cash transactions, and therefore IV estimates should be preferred. Table 3.2 shows the results of the second stage IV regressions and the identification tests.

Table 3.2. Second Stage FE IV Regression of VAT Gap

	Log (VAT gap)	VAT gap as % of GDP	VAT gap as % of VTTL
Log (Card payments as % of GDP)	-0.3387	-0.5107*	-7.6172**
	(0.274)	(0.290)	(3.362)
Log (Cash withdrawals as % of GDP)	0.4259**	0.5951***	4.3074**
	(0.168)	(0.192)	(1.840)
Share of whole and retail sale in VA (%)	-0.0195	-0.0502	0.1004
	(0.026)	(0.036)	(0.279)
Share of real estate in VA (%)	-0.0956**	-0.0685*	-0.7450**
	(0.043)	(0.039)	(0.363)
Share of arts in VA (%)	-0.0589	0.0525	0.2820
	(0.060)	(0.062)	(0.610)
Dispersion rate	-0.3296	4.1564***	38.0624***
	(1.392)	(1.387)	(14.426)
Unemployment rate	0.0123	0.0350**	0.2963*
	(0.013)	(0.017)	(0.160)
Age structure	0.0347	0.0743	0.3829
	(0.036)	(0.046)	(0.406)
Deficit to GDP ratio	-0.0282**	-0.0298**	-0.2804*
	(0.014)	(0.015)	(0.144)
Log (GDP per capita)	0.8588**		4.4696
	(0.357)		(3.775)
R-squared	0.264	0.254	0.176
Observations	395	395	395
Number of countries	25	25	25
Cragg-Donald Wald F	15.77	24.02	15.77
Kleibergen-Paap rk Wald F	15.21	24.10	15.21
Hansen J	1.552	1.510	5.584
Prob > J	0.670	0.680	0.134
C test of endogeneity	11.22	22.70	24.86
Prob > C	0.0037	0.0000	0.0000

Notes: The following regressors are omitted from presentation: share of construction in VA, share of industry in VA, share of telecommunications in VA, and Log(Population). All regressions include country, and year fixed effects. Robust standard errors appear in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Conversely to the naïve OLS estimates, it appears that card payments are somewhat contributive in reducing tax evasion. In fact, column (3) reports a strong negative effect of plastic money use on VAT evasion at the 5% significance level while column (2) shows a less strong effect (significant at the 10% level) of card payments on tax compliance. Specifically, the analysis finds that a 1% increase in card payments as share of GDP translates to 0.51 percentage point smaller VAT gap as percent of GDP. This suggests that doubling the share of card transactions in output would halve the share of VAT gap in output. As for the VAT gap as a percent of VTTL, the estimates show that a 1% rise in plastic money use is associated with a 7.6 percentage point improvement in compliance. Therefore, ceteris paribus, to halve the share of the VAT gap in the value-added total tax liability, plastic money use needs to increase by roughly 6.6%. The IV regression also reveals a strong positive and consistent effect of cash withdrawals on tax evasion across all the specifications. The results in column (2) indicates that a 1% rise in the share of cash withdrawals in output would lead to an approximately 0.43% increase in the VAT gap or roughly 0.6% as percent of GDP. When expressed as a percent of the VTTL, a 1% increase in cash withdrawals is linked to a 4.3 percentage points increase in evasion. However, it should be noted that the estimated effect for the log specification in column (1) is negative yet not significant. A potential explanation for the difference in statistical significance to columns (2) and (3) is the fact the outcome in (1) is not normalized. Still, this imprecisely estimated, negative effect may not be entirely dismissed as null as it is not centered around zero. These findings are largely in line with Immordino and Russo (2018a) who reported that plastic money use improves VAT compliance. These results are in line with previous studies which found that cash withdrawals foster tax evasion (Immordino and Russo, 2018a; Madzharova, 2020).

It also appears that the productive structure of the economy is a key determinant of tax compliance. In fact, the IV estimates show that an increased share of the real estate sector in the total value added of the economy is associated with lower VAT gaps in all the three specifications. This could be explained by the fact that real estate transactions typically involve substantial amount of money in which case the use of cash is nearly prohibitive, if not actually outlawed. Moreover, those transactions involve extensive paper trail in the form of contracts and land registration.

The results also show some large, positive and significant effect of the dispersion of tax rates on tax evasion. The greater the dispersion, the higher the benefits from a misapplication of reduced rates, which can widen the VAT compliance gap. The IV estimates also report large increases in the VAT gap by 4.15 percentage points of the GDP when the dispersion rate is increased by one unit. When considering an even narrower basis in VTTL, the effect is magnified further to 38 percentage points of the VTTL. These results illustrate the importance of a singular VAT rate within EU countries as also highlighted in Hopland and Ullman (2019) for the case of Germany. Finally, the estimation reports a small, negative and significant association between the deficit to GDP ratio and VAT gaps, suggesting that a one-unit increase of the deficit to GDP ratio will decrease VAT gaps by 0.028 percentage points of the GDP. As stated earlier, ex ante efforts made by tax administrations to close the VAT gap can be driven by the level of public indebtedness. A higher level of deficit would likely pressure tax authorities to take measures that can have a deterrent effect on private agents' tax evasion behavior.

3.5. Conclusion

There is evidence that card payments' traceability could enhance VAT compliance by increasing the perceived probability of detection by the tax authorities which enhances the VAT's deterrence effect (Pomeranz, 2015). This paper investigates the effect of payment methods on valued-added tax compliance in EU countries. It differs from most of the earlier works first by considering a more appropriate measure of the compliance gap that nets out the policy gap given that the entire gap is not made up of tax fraud or evasion. Therefore, a concern with most of these earlier studies is the use of VAT revenue or VRR as measure of tax revenue efficiency without controlling in the regressions for the effect of policy choices on tax structure, the enforcement capacity of the tax administration, etc. Some of these factors cannot even be easily accounted for when using aggregate level data, possibly leading to omitted variable bias issues in the estimates of those papers. Second, even though one of the previous works used the appropriate measures of VAT gap, it examines the effect of one means of payment at a time, a potential for omitted variable bias. In addition, using adequate measures of VAT compliance gap along with more recent data enables us to obtain updated estimates of the relationship between plastic money use and tax compliance.

Using a fixed effect panel data estimation and IV techniques on 26 EU countries from 2000 to 2016, it appears that card payments statistically positively affect tax revenue collection efficiency. At the antipodes of Hondroyiannis and Papaoikonomou (2020) and Madzharova (2020), the OLS results confirm Immordino and Russo (2018a) who find that plastic money use does not translate into more VAT revenue collection in a naïve OLS regression. In addition, this study corroborates Madzharova (2020) and Immordino and Russo (2018a) who report that cash transactions are associated with a significant reduction in VAT revenue collected. This paper

argues that its approach provides a more adequate assessment of the relationship between plastic money use and VAT compliance gap relative to Immordino and Russo (2018a) by dealing simultaneously with both means of payments. It also explores a potential endogeneity problem inherent to the choice of a specific means of transactions, which could bias the cross-country analysis. In line with Immordino and Russo (2018a), this paper finds some evidence of the effect of credit and debit card payments on VAT compliance gap. These results suggest that the increased use of plastic money led to gains in consumption tax compliance. However, the non-significant result for the log specification may require further investigation. To completely close the debate on this topic, a future research program would require micro-level data or administrative records. In other fields, it has been documented that aggregate-level analyses might be limited in power to estimate statistically significant effects (Black et al, 2019). In addition, it would be interesting to explore the impact of the spike in the use of electronic forms of payments due to the exogenous shift in norms in response to COVID-19 which imposes confinement and social distancing. To investigate this, updated VAT gap estimates are needed once member countries' national administrations release their respective end-of-year estimates.

The results regarding the use of plastic money and especially cash transactions have potential wider policy implications for both national governments and European authorities**.** Cash transactions have the property of preserving the anonymity of the payer and recipient and so, despite their overall convenience, they can be used for fraudulent and criminal purposes. As aforementioned, the EU has investigated how cash transactions have not only contributed to fraudulent activities but also to financing terrorism (Europol, 2015). Imposing cash threshold limits has been already introduced in a number of EU countries to combat these problems, but the lack of policy uniformity (for example, a EU maximum uniform cash threshold) is believed to be

adversely affecting the effectiveness of those measures in any country, thus, the consideration of a uniform standard in the EU.[9] Based on the adverse impact of (within-country) tax rate dispersion on VAT gaps, it could be argued that the application of a uniform VAT rate has the potential of generating very large reductions in the VAT gap. Lastly, it is worth mentioning that these EU-based findings may very well be generalizable to other countries around the world since they are based on a mix of developed and transition economies.

[9] For the list of cash payment limit thresholds in the EU, see: https://www.europe-consommateurs.eu/fileadmin/user_upload/eu-consommateurs/PDFs/PDF_EN/Limit_for_cash_payments_in_EU.pdf

Appendices

Appendix A. Effect of the FPEP on School Outcomes

Table A.1. Test of Common Pre-Reform Trends Using Private Schools as Control

	Log (6th Graders)			Log (Graduates)			Graduation Rate		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All									
Public school * Year = 2005	0.0387	0.0802	0.0640	0.0819	0.1308	0.0743	0.0035	0.0024	-0.0027
	(0.071)	(0.064)	(0.061)	(0.123)	(0.144)	(0.140)	(0.019)	(0.022)	(0.023)
Public school * Year = 2006	0.0211	0.0211	-0.0071	0.1313	0.1313	0.0654	-0.0117	-0.0117	-0.0164
	(0.030)	(0.036)	(0.034)	(0.085)	(0.101)	(0.092)	(0.016)	(0.018)	(0.019)
Public school * Year = 2007	0.0214	0.0214	0.0091	0.0323	0.0323	0.0342	0.0105	0.0105	0.0115
	(0.029)	(0.035)	(0.035)	(0.063)	(0.075)	(0.080)	(0.022)	(0.027)	(0.028)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	327	327	327	327	327	327	327	327	327
R-squared	0.817	0.994	0.994	0.751	0.949	0.953	0.553	0.755	0.757
Panel B: Boys									
Public school * Year = 2005	0.0592	0.1112	0.1010	0.0917	0.1443	0.0913	0.0099	0.0087	0.0026
	(0.072)	(0.072)	(0.068)	(0.132)	(0.145)	(0.142)	(0.019)	(0.022)	(0.022)
Public school * Year = 2006	0.0249	0.0249	0.0134	0.1125	0.1125	0.0461	-0.0128	-0.0128	-0.0179
	(0.032)	(0.038)	(0.037)	(0.080)	(0.095)	(0.094)	(0.015)	(0.018)	(0.019)
Public school * Year = 2007	0.0396	0.0396	0.0394	0.0034	0.0034	-0.0003	0.0139	0.0139	0.0153
	(0.031)	(0.037)	(0.040)	(0.059)	(0.070)	(0.078)	(0.023)	(0.028)	(0.029)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	327	327	327	327	327	327	327	327	327
R-squared	0.798	0.993	0.993	0.743	0.952	0.955	0.551	0.740	0.743
Panel C: Girls									
Public school * Year = 2005	0.0035	0.0344	0.0165	0.0717	0.1151	0.0594	-0.0016	-0.0028	-0.0064
	(0.078)	(0.073)	(0.071)	(0.126)	(0.151)	(0.148)	(0.022)	(0.026)	(0.027)
Public school * Year = 2006	0.0103	0.0103	-0.0261	0.1523	0.1523	0.0908	-0.0060	-0.0060	-0.0102
	(0.043)	(0.051)	(0.056)	(0.097)	(0.115)	(0.102)	(0.021)	(0.025)	(0.026)
Public school * Year = 2007	0.0098	0.0098	-0.0103	0.0714	0.0714	0.0793	0.0104	0.0104	0.0108
	(0.048)	(0.057)	(0.059)	(0.080)	(0.094)	(0.095)	(0.027)	(0.032)	(0.033)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	327	327	327	327	327	327	327	327	327
R-squared	0.834	0.990	0.991	0.751	0.940	0.944	0.539	0.762	0.762

Notes: Standard errors, heteroscedasticity-robust and clustered by district, are reported in parentheses. All regressions include public school dummy, district, and year fixed effects. Control variables include the number of schools per enrollee, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.2. Placebo Test of Common Pre-Reform Trends Using Private Schools as Control

	Log (6th Graders)			Log (Graduates)			Graduation Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: All									
Public school * Post	-0.019	-0.040	-0.024	-0.091	-0.115	-0.053	0.009	0.010	0.015
	(0.038)	(0.041)	(0.037)	(0.076)	(0.092)	(0.086)	(0.011)	(0.013)	(0.015)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
District trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School type-district FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	327	327	327	327	327	327	327	327	327
R-squared	0.817	0.993	0.994	0.751	0.949	0.952	0.553	0.755	0.756
Panel B: Boys									
Public school * Post	-0.022	-0.048	-0.038	-0.100	-0.127	-0.069	0.009	0.009	0.015
	(0.043)	(0.043)	(0.040)	(0.076)	(0.091)	(0.085)	(0.011)	(0.013)	(0.015)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
District trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School type-district FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	327	327	327	327	327	327	327	327	327
R-squared	0.798	0.992	0.992	0.743	0.952	0.955	0.549	0.739	0.742
Panel C: Girls									
Public school * Post	-0.002	-0.017	-0.001	-0.077	-0.098	-0.035	0.009	0.01	0.014
	(0.041)	(0.047)	(0.044)	(0.082)	(0.100)	(0.093)	(0.013)	(0.015)	(0.017)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
District trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
School type-district FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	327	327	327	327	327	327	327	327	327
R-squared	0.834	0.990	0.991	0.751	0.94	0.944	0.539	0.761	0.762

Notes: Standard errors, heteroscedasticity-robust and clustered at the district level, are reported in parentheses. All regressions include public school dummy, district, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.3. Effect on Demand Using Private Schools as Control – Negative Binomial

	6th Graders		Graduates	
	(1)	(2)	(3)	(4)
Panel A: All				
Public school * Post	0.2385***	0.2500***	0.1540**	0.1505**
	(0.050)	(0.050)	(0.063)	(0.060)
Controls	No	Yes	No	Yes
District trends	Yes	Yes	Yes	Yes
School type-district FE	Yes	Yes	Yes	Yes
Observations	1061	1061	1061	1061
Panel B: Boys				
Public school * Post	0.2465***	0.2590***	0.1583***	0.1575***
	(0.049)	(0.048)	(0.059)	(0.057)
Controls	No	Yes	No	Yes
District trends	Yes	Yes	Yes	Yes
School type-district FE	Yes	Yes	Yes	Yes
Observations	1061	1061	1061	1061
Panel C: Girls				
Public school * Post	0.2420***	0.2508***	0.1701**	0.1563**
	(0.053)	(0.054)	(0.074)	(0.069)
Controls	No	Yes	No	Yes
District trends	Yes	Yes	Yes	Yes
School type-district FE	Yes	Yes	Yes	Yes
Observations	1061	1061	1061	1061

Notes: Standard errors, heteroscedasticity-robust and clustered at the district level, are reported in parentheses. All regressions include public school dummy, district, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.4. Effect on Supply Using Private Schools as Control – Negative Binomial

	Schools		Classrooms		Teachers	
	(1)	(2)	(3)	(4)	(5)	(6)
Public school * Post	0.4769***	0.4077***	0.3499***	0.3202***	0.3434***	0.3288***
	(0.053)	(0.056)	(0.050)	(0.049)	(0.046)	(0.048)
District trends	No	Yes	No	Yes	No	Yes
School type-district FE	No	Yes	No	Yes	No	Yes
Observations	1061	1061	1061	1061	1061	1061

Notes: Standard errors, heteroscedasticity-robust and clustered at the district level, are reported in parentheses. All regressions include public school dummy, district, and year fixed effects. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.5. Test of Common Pre-Reform Trends Across Prefectures and School Type

	Log (6th Graders)		Log (Graduates)		Graduation Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Intensity*Private						
Year = 2006	-0.0086	0.0005	-0.0266	-0.0154	0.0016	0.0014
	(0.009)	(0.005)	(0.021)	(0.020)	(0.003)	(0.003)
Year = 2007	-0.0037	-0.0038	-0.0267	-0.0261	0.0007	0.0010
	(0.007)	(0.006)	(0.021)	(0.021)	(0.002)	(0.003)
Year = 2008	-0.0038	-0.0030	-0.0146	-0.0136	0.0005	0.0005
	(0.010)	(0.009)	(0.020)	(0.020)	(0.003)	(0.003)
Intensity*Public						
Year = 2006	-0.0002	-0.0009	0.0013	-0.0008	0.0009	0.0005
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
Year = 2007	-0.0003	-0.0019	0.0059*	0.0035	0.0030	0.0029
	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)
Year = 2008	0.0040	0.0010	0.0042	-0.0012	-0.0006	-0.0011
	(0.003)	(0.003)	(0.005)	(0.004)	(0.002)	(0.002)
Controls	No	Yes	No	Yes	No	Yes
Observations	271	271	271	271	271	271
R-squared	0.863	0.880	0.801	0.821	0.610	0.623
Panel B: Boys						
Intensity*Private						
Year = 2006	-0.0099	0.0003	-0.0261	-0.0138	0.0013	0.0011

	(0.010)	(0.005)	(0.021)	(0.020)	(0.002)	(0.003)
Year = 2007	-0.0096	-0.0092	-0.0253	-0.0250	0.0010	0.0013
	(0.012)	(0.009)	(0.021)	(0.020)	(0.002)	(0.002)
Year = 2008	-0.0027	-0.0017	-0.0180	-0.0169	0.0001	0.0001
	(0.009)	(0.008)	(0.020)	(0.020)	(0.002)	(0.002)
Intensity*Public						
Year = 2006	-0.0005	-0.0010	0.0002	-0.0017	0.0009	0.0005
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
Year = 2007	-0.0002	-0.0019	0.0042	0.0017	0.0029	0.0029
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Year = 2008	0.0041	0.0011	0.0041	-0.0013	-0.0001	-0.0006
	(0.003)	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)
Controls	No	Yes	No	Yes	No	Yes
Observations	271	271	271	271	271	271
R-squared	0.847	0.869	0.793	0.818	0.609	0.621
Panel C: Girls						
Intensity*Private						
Year = 2006	-0.0069	0.0013	-0.0259	-0.0158	0.0020	0.0017
	(0.008)	(0.004)	(0.022)	(0.021)	(0.003)	(0.003)
Year = 2007	0.0004	0.0000	-0.0274	-0.0265	0.0003	0.0006
	(0.005)	(0.004)	(0.022)	(0.022)	(0.003)	(0.003)
Year = 2008	-0.0049	-0.0043	-0.0117	-0.0107	0.0010	0.0010
	(0.010)	(0.010)	(0.023)	(0.023)	(0.003)	(0.003)
Intensity*Public						
Year = 2006	0.0008	0.0001	0.0038	0.0013	0.0011	0.0006
	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)	(0.002)
Year = 2007	-0.0002	-0.0018	0.0098**	0.0075	0.0033	0.0032
	(0.003)	(0.002)	(0.004)	(0.005)	(0.002)	(0.002)
Year = 2008	0.0042	0.0013	0.0045	-0.0010	-0.0015	-0.0020
	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)
Controls	No	Yes	No	Yes	No	Yes
Observations	271	271	271	271	271	271
R-squared	0.875	0.889	0.800	0.815	0.596	0.610

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.6. Placebo Test of Common Pre-Reform Trends Across Prefectures and School Type

	Log (6th Graders)		Log (Graduates)		Graduation Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Intensity*Post*Private	0.0010	-0.0037	-0.0059	-0.0114	-0.0003	0.0000
	(0.003)	(0.007)	(0.010)	(0.010)	(0.001)	(0.002)
Intensity*Post*Public	0.0020	-0.0000	0.0044*	0.0014	0.0007	0.0007
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	271	271	271	271	271	271
R-squared	0.862	0.880	0.796	0.818	0.602	0.614
Panel B: Boys						
Intensity*Post*Private	-0.0007	-0.0056	-0.0072	-0.0134	-0.0002	0.0001
	(0.004)	(0.009)	(0.009)	(0.011)	(0.001)	(0.001)
Intensity*Post*Public	0.0022	0.0001	0.0041*	0.0010	0.0010	0.0009
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	271	271	271	271	271	271
R-squared	0.846	0.868	0.789	0.816	0.601	0.612
Panel C: Girls						
Intensity*Post*Private	0.0016	-0.0029	-0.0051	-0.0099	-0.0004	-0.0001
	(0.003)	(0.007)	(0.012)	(0.011)	(0.002)	(0.002)
Intensity*Post*Public	0.0016	-0.0002	0.0053*	0.0025	0.0003	0.0003
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	271	271	271	271	271	271
R-squared	0.874	0.888	0.794	0.811	0.585	0.598

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.7. Effect on Demand Across Prefectures and School Type – Negative Binomial

	6th Graders		Graduates	
	(1)	(2)	(3)	(4)
Panel A: All				
Intensity*Post*Private	0.0133***	0.0101**	0.0002	-0.0025
	(0.003)	(0.005)	(0.008)	(0.008)
Intensity*Post*Public	0.0184***	0.0167***	0.0132***	0.0115***
	(0.002)	(0.004)	(0.003)	(0.004)
Controls	No	Yes	No	Yes
Observations	880	880	880	880
Panel B: Boys				
Intensity*Post*Private	0.0124***	0.0094**	0.0008	-0.0019
	(0.004)	(0.004)	(0.007)	(0.008)
Intensity*Post*Public	0.0173***	0.0153***	0.0132***	0.0113***
	(0.002)	(0.004)	(0.003)	(0.004)
Controls	No	Yes	No	Yes
Observations	880	880	880	880
Panel C: Girls				
Intensity*Post*Private	0.0148***	0.0117***	-0.0007	-0.0034
	(0.003)	(0.005)	(0.009)	(0.009)
Intensity*Post*Public	0.0211***	0.0198***	0.0140***	0.0129***
	(0.003)	(0.004)	(0.004)	(0.005)
Controls	No	Yes	No	Yes
Observations	880	880	880	880

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.8. Effect on Demand Across Prefectures and School Type – Average Enrollment Rates

	Log (6th Graders)		Log (Graduates)		Graduation Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Intensity*Post*Private	0.0128***	0.0080	0.0094*	0.0046	-0.0004	-0.0002
	(0.004)	(0.005)	(0.005)	(0.006)	(0.001)	(0.001)
Intensity*Post*Public	0.0184***	0.0169***	0.0140***	0.0128***	-0.0010	-0.0009
	(0.003)	(0.003)	(0.003)	(0.004)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	880	880	880	880	880	880
R-squared	0.859	0.867	0.817	0.826	0.480	0.485
Panel B: Boys						
Intensity*Post*Private	0.0130***	0.0075	0.0099*	0.0043	-0.0005	-0.0003
	(0.004)	(0.005)	(0.006)	(0.006)	(0.001)	(0.001)
Intensity*Post*Public	0.0172***	0.0155***	0.0136***	0.0121***	-0.0008	-0.0007
	(0.002)	(0.003)	(0.003)	(0.004)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	880	880	880	880	880	880
R-squared	0.849	0.859	0.809	0.820	0.466	0.471
Panel C: Girls						
Intensity*Post*Private	0.0137***	0.0095*	0.0100*	0.0060	-0.0002	0.0001
	(0.003)	(0.005)	(0.005)	(0.007)	(0.002)	(0.002)
Intensity*Post*Public	0.0210***	0.0198***	0.0156***	0.0146***	-0.0012	-0.0010
	(0.003)	(0.004)	(0.004)	(0.005)	(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes
Observations	880	880	880	880	880	880
R-squared	0.865	0.872	0.818	0.824	0.482	0.488

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Control variables include the number of school per student, the count of classroom per student, and the count of classroom per teacher. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.9. Effect on Supply Across Prefectures and School Type – Negative Binomial

	Schools	**Classrooms**	**Teachers**
Intensity*Post*Private	0.0019	0.0027	0.0093**
	(0.003)	(0.003)	(0.005)
Intensity*Post*Public	0.0148***	0.0139***	0.0134***
	(0.004)	(0.003)	(0.003)
Observations	880	880	880

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table A.10. Effect on Supply Across Prefectures and School Type – Average Enrollment Rates

Panel A	**Log (Schools)**	**Log (Classrooms)**	**Log (Teachers)**
Intensity*Post*Private	0.0020	0.0033	0.0049
	(0.004)	(0.004)	(0.004)
Intensity*Post*Public	0.0147***	0.0140***	0.0135***
	(0.003)	(0.003)	(0.002)
Observations	880	880	880
R-squared	0.836	0.845	0.837

Panel B	**School/student**	**Classroom/student**	**Teacher/student**
Intensity*Post*Private	-0.0000	-0.0001	0.0001
	(0.000)	(0.000)	(0.000)
Intensity*Post*Public	-0.0000	-0.0001*	-0.0001**
	(0.000)	(0.000)	(0.000)
Observations	880	880	880
R-squared	0.663	0.566	0.21
Pre-reform mean	0.006	0.028	0.027

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include public school dummy, prefecture, and year fixed effects. Significance levels: *** = 1%; ** = 5%; * = 10%.

Appendix B. Effect of the FPEP on Child Labor

Table B.1. Effect on Child Non-Economic Activities

	All Children		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	-0.0391**	-0.0355*	-0.0255	-0.0257	-0.0512*	-0.0491*
	(0.019)	(0.018)	(0.023)	(0.023)	(0.028)	(0.028)
Treat	0.0500***	0.0464***	0.0439***	0.0478***	0.0526***	0.0530***
	(0.009)	(0.009)	(0.014)	(0.017)	(0.015)	(0.019)
Girl		0.0475***				
		(0.007)				
Son or daughter of the household head		-0.0033		-0.0013		-0.0052
		(0.012)		(0.017)		(0.015)
Genitrix lives in the household		-0.0249**		-0.0201**		-0.0286*
		(0.011)		(0.010)		(0.016)
Mother's education level		0.0208**		0.0311***		0.0090
		(0.008)		(0.009)		(0.012)
Education level of the household head		-0.0049		-0.0063		-0.0036
		(0.006)		(0.007)		(0.007)
Wealth index score		-0.0162**		-0.0235**		-0.0077
		(0.008)		(0.010)		(0.008)
Log(Size of the household)		-0.0029		0.0121		-0.0186
		(0.012)		(0.011)		(0.015)
Rural area		-0.0008		-0.0116		0.0128
		(0.014)		(0.018)		(0.016)
Observations	19057	19057	9673	9673	9384	9384

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include prefecture, and year fixed effects. AMEs are reported. Significance levels: *** = 1%; ** = 5%; * = 10%.

Table B.2. Effect on Child Economic Activities

	All Children		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	0.0200	0.0120	0.0314	0.0282	0.0117	-0.0009
	(0.025)	(0.026)	(0.027)	(0.028)	(0.052)	(0.052)
Treat	0.1348***	0.1234***	0.1349***	0.1391***	0.1316***	0.1414***
	(0.011)	(0.012)	(0.019)	(0.028)	(0.016)	(0.024)
Girl		-0.0013				
		(0.011)				
Son or daughter of the household head		-0.0075		-0.0087		-0.0048
		(0.011)		(0.019)		(0.016)
Genitrix lives in the household		-0.0068		-0.0129		0.0002
		(0.012)		(0.019)		(0.015)
Mother's education level		-0.0168		-0.0105		-0.0221**
		(0.012)		(0.016)		(0.011)
Education level of the household head		-0.0219***		-0.0135		-0.0290***
		(0.008)		(0.009)		(0.007)
Wealth index score		-0.0468***		-0.0645***		-0.0310*
		(0.013)		(0.013)		(0.016)
Log(Size of the household)		0.0415***		0.0299**		0.0512***
		(0.012)		(0.015)		(0.012)
Rural area		0.0509**		0.0493**		0.0529*
		(0.022)		(0.024)		(0.027)
Observations	19096	19096	9695	9695	9401	9401

Notes: Standard errors, heteroscedasticity-robust and clustered at prefecture level, are reported in parentheses. All regressions include prefecture, and year fixed effects. AMEs are reported. Significance levels: *** = 1%; ** = 5%; * = 10%.

Appendix C. Impact of Plastic Money Use on VAT Compliance

Table C.1. Summary Statistics and Data Sources

Variables	Source	Max	Min	Mean	Std. Dev.	N
VAT gap (in EUR million)	2013 & 2018 TAXUD report	40424	20	5630.30	8652.70	442
VAT gap as % of VTTL	2018 TAXUD report	49	0	16.4522	10.2644	442
VAT gap as % of GDP	2018 TAXUD report	7.9	0	1.5903	1.2437	442
Card payments as % of GDP	European Central Bank	0.46	0.001	0.11	0.08	437
ATM cash withdrawals as % of GDP	European Central Bank	0.30	0.01	0.13	0.06	413
Number of ATM per million inhabitants	European Central Bank	1667.63	33.46	666.59	333.98	441
Share of whole and retail sale in VA (%)	Eurostat	32.35	10.03	20.85	3.64	442
Share of real estate in VA (%)	Eurostat	19.13	4.91	9.34	2.46	442
Share of construction in VA (%)	Eurostat	12.66	1.02	6.22	1.71	442
Share of industry in VA (%)	Eurostat	38.58	6.15	21.33	5.6	442
Share of telecommunication in VA (%)	Eurostat	10.59	3.02	4.84	1.07	442
Share of arts in VA (%)	Eurostat	14.57	1.4	3.07	1.41	442
Dispersion of tax rates	Authors, 2018 TAXUD report	0.12	0	0.07	0.03	442
Scale of the tax administration	OECD	0.01	0	0	0	304
IT expenditure over total administrative costs	OECD	0.29	0	0.1	0.07	208
Unemployment rate (%)	Eurostat	27.5	1.8	8.86	4.35	442
Government effectiveness index	World Bank	2.35	-0.37	1.17	0.62	416
Broadband Internet subscriptions for 100 people	World Bank	43.10	0.01	18.96	12.13	425
Age structure (%)	Eurostat	43.2	25.7	35.15	3.37	442
Deficit to GDP ratio as % of GDP	Eurostat	6.9	-32.1	-2.65	3.66	442
GDP per capita (in EUR)	Eurostat	91300	1800	23655.91	16068.98	440
Population (in million)	Eurostat	82.53	0.39	19.02	23.02	442
Size of the shadow economy (%)	Medina & Schneider (2018)	35.30	7.69	18.13	6.72	416

Table C.2. FE Panel Regression of VAT Gap Including CEE Country Dummy

	Log (VAT gap)		VAT gap as % of GDP		VAT gap as % of VTTL	
Log(Card payments as % of GDP)	0.0482	0.0734	0.1030	0.4390	0.5584	4.2218
	(0.137)	(0.320)	(0.155)	(0.276)	(1.566)	(2.594)
Log(ATM cash withdrawals as % of GDP)	-0.0887	-0.0863	-0.1500	-0.1119	-2.5835	-2.2439
	(0.224)	(0.228)	(0.140)	(0.151)	(1.654)	(1.740)
Log(Card payments as % of GDP) x CEE dummy		-0.0286		-0.3495		-4.1581
		(0.320)		(0.292)		(3.063)
Share of whole and retail sale in VA (%)	-0.0148	-0.0152	-0.0562	-0.0617	0.0526	0.0064
	(0.030)	(0.030)	(0.056)	(0.054)	(0.391)	(0.377)
Share of real estate in VA (%)	-0.0614	-0.0619	-0.0195	-0.0271	-0.1995	-0.2761
	(0.055)	(0.057)	(0.056)	(0.054)	(0.510)	(0.503)
Share of arts in VA (%)	-0.0754	-0.0780	0.0347	0.0066	0.0372	-0.3384
	(0.064)	(0.075)	(0.055)	(0.057)	(0.530)	(0.584)
Dispersion of tax rates	-0.7873	-0.8202	2.5325	2.1210	26.1645	21.3974
	(1.814)	(1.806)	(1.940)	(1.859)	(18.965)	(18.681)
Unemployment rate	0.0241	0.0240	0.0554**	0.0526**	0.4771**	0.4628**
	(0.014)	(0.015)	(0.023)	(0.025)	(0.206)	(0.210)
Age structure	0.0750	0.0750	0.1367**	0.1369**	1.0273*	1.0295*
	(0.051)	(0.052)	(0.059)	(0.060)	(0.561)	(0.567)
Deficit to GDP ratio as % of GDP	-0.0297	-0.0297	-0.0323*	-0.0328*	-0.3296*	-0.3217*
	(0.019)	(0.019)	(0.018)	(0.018)	(0.176)	(0.177)
Log(Population)	-0.3827	-0.4238	1.0475	0.2753	8.0680	2.1029
	(2.548)	(2.570)	(1.728)	(1.739)	(17.840)	(18.689)
Log(GDP per capita)	0.9493*	0.9645*			1.8887	4.1014
	(0.463)	(0.509)			(3.817)	(4.454)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	412	412	412	412	412	412
R-squared	0.317	0.317	0.314	0.318	0.286	0.292
Number of countries	26	26	26	26	26	26

Notes: The following regressors are omitted from presentation: share of construction in VA, share of industry in VA, share of telecommunications in VA, Log(population) and the constant term. Robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C.3. First Stage FE IV Regression Estimates

	Log(Card payments over GDP)	Log(ATM cash withdrawals over GDP)
Log(# ATM per million inhabitants)	0.270***	0.684***
	(0.0797)	(0.0803)
Broadband	0.0586***	-0.0491**
	(0.0143)	(0.0199)
Log(# ATM per million inhabitants)*Broadband	-0.00944***	0.00667**
	(0.00207)	(0.00294)
Bank Branches per 100 km^2	0.103*	0.143**
	(0.0605)	(0.0627)
Log(#ATM per million inhabitants)*Bank Branches per 100 km^2	-0.00912	-0.0211**
	(0.00899)	(0.00952)
R-squared	0.948	0.885
Observations	400	395
Number of countries	25	25
Country FE	Yes	Yes
Year FE	Yes	Yes

Notes: The following regressors are omitted from presentation: share of whole and retail sale in VA, share of real estate in VA, share of construction in VA, share of industry in VA, share of telecommunications in VA, share of arts, dispersion rate, unemployment rate, age structure, deficit to GDP ratio, Log(population), Log(GDP per capita), and the constant term. Robust standard errors appear in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

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